





BRAITHWAITE'S RETROSPECT.

VOL. LXX. JULY—DECEMBER, 1874.



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THE
RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND
PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

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LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN
AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

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SYNOPSIS.

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

AFFECTIONS OF THE SYSTEM GENERALLY.

ACUTE RHEUMATISM.—*Immovable Bandages*.—Favourable results are obtained in acute rheumatism, as regards the pain, the fever, and the duration of the disease, by fixing the parts immovably, and so maintaining them not only until all pain and swelling have left the joint, but until the constitutional symptoms have disappeared, and especially until the temperature has returned to the normal standard. This may be effected by means of pasteboard splints, or for small children and restless patients by plaster of Paris. (Dr. Oehme, p. 34.)

LUMBAGO.—*Guarana*.—Fifteen grains of guarana in hot water, with a little cream and sugar, is a remedy worth trying in cases of lumbago. If necessary forty grains may be taken regularly once a day. The results vary according to the nature of the case. I have come to the following conclusion, viz., that whenever the fibrous envelopes of nerves, the aponeurotic sheath of muscles, the fasciæ or tendons are the parts affected, guarana gives if not instantaneous, at least very immediate relief, which will last from twelve to twenty-four hours; and I confidently expect that perseverance in the use of the drug, gradually increasing the dose up to 40 grs., will entirely remove any of the above-mentioned kinds of rheumatism. Of the good effects of guarana on nervous hemicrania there is no doubt; and I trust it will prove, in other hands, as valuable against rheumatism as it has in mine. (Dr. E. A. Rawson, p. 33.)

TYPHOID FEVER.—The so-called pythogenic theory cannot in the majority of cases be for a moment entertained. We are all acquainted with places constantly saturated with evil odours of all kinds, including not unfrequently that of decomposing faecal matter, and yet there is no fever. Horrible though it may seem, these matters make their way into

drinking water, and yet no typhoid fever follows. To excite an outbreak of typhoid something more is necessary, and the question is—What is this something? Without doubt it has been shown by the researches of Drs. Budd, Ballard, and many others that the special exciting cause is typhoid excreta; and if so in a certain number of instances, there is a strong probability that it is so in all. In the typhoid stools undoubtedly are to be found the germs which propagate typhoid in a certain number of instances; and if putrid animal matter will not alone generate typhoid, it certainly constitutes a nidus, in which these germs thrive and apparently retain their vitality for a long period of time. The nature of these germs is unknown to us, but inasmuch as they retain their power of self-propagation, they must consist of particles more or less minute of living matter. (Editor of Medical Times and Gazette, p. 8.)

Several interesting facts are related, showing that the exclusive contagion theory of the origin of typhoid is untenable. Any decomposing or putrid animal matter, especially when dissolved or suspended in water, may produce the disease when taken into the stomach, as drinking water; or if exhaling into the atmosphere, by entering the lungs. (Dr. W. Strange, p. 17.)

The author, after relating a case of typhoid, considers the following conclusions proved:—That the constant exposure to sewage emanations appears to protect the system in some degree against the effects of typhoid virus. That, in some instances, individuals after a change of residence from a healthy locality to one favourable to typhoid fever are more susceptible to the poison than those long resident in it. That the sewage emanations in this place were no doubt the cause of this attack, and that the stage of incubation was from fifteen to twenty days. That sewage emanations are sufficient to create and produce typhoid fever, though not contaminated with the discharge of typhoid patients. (Dr. A. Mackintosh, p. 23.)

The Eruption of Typhoid Fever.—One of the most characteristic features of typhoid is the eruption which generally appears upon the skin. The spots are of a pale rose colour, and fade on pressure. In size they vary from half a millet seed to less than that of a split pea. They are rounded though not distinctly margined, are flattened on the surface, and give no feeling of hardness or resistance to the finger passed over them. They are not always present, but are generally so, and appear in crops of from three to six at a time, each spot remaining out for three or four days. (Dr. T. B. Peacock, p. 1.)

Treatment.—The poison of typhoid fever is almost certainly mainly propagated in the alimentary canal, and is therefore, much more than ordinary animal poisons, within the reach of medicaments. Experience has shown that certain substances, like green sulphate of copper, creasote, and carbolic acid, Condý's fluid, &c., have the power of destroying the germs of the disorder outside the body. Why should they not be used inwardly? To this the reply is, that there is no particular reason why they should not in moderate quantity: but experience is wanting as to their efficacy in arresting the production of fever germs, or in destroying those already formed. Another suggestion, however, occurs; it is well known that the pea-soupy stools of confirmed typhoid are invariably alkaline, and it is a question how far the germs which these contain can exist in presence of a strong acid. Moreover, experience declares itself on the side of giving acids in the disease, since they tend to remove the clammy condition of tongue which adds so greatly to the sensation of thirst, and to prevent severe diarrhœa. For these latter reasons alone we should be justified *nihil contradicente* to order dilute or aromatic sulphuric acid, or dilute hydrochloric or nitrohydrochloric acid in ordinary doses from time to time, having a hope that the same remedies might tend to limit the spread of the disease by limiting the growth and number of the typhoid germs. Having, however, no assured means of cutting the malady short, we are obliged to confine ourselves to treating the complications of the disease. Foremost among these is excessive temperature. It seems to us that temperatures under 104° Fahr. may as a rule be dealt with by increasing the quantity of alcohol given to the patient, by tepid sponging of the surface, and especially by the giving of quinine. As to the administration of alcohol, we hardly think it possible to lay down rules to be generally available. Sponging is simpler, and is available at anything over 100° Fahr. But to give quinine effectually it must be given in full dose; as a rule we begin with twenty grains, and continue for some time thereafter to give five grains every four hours. This, no doubt, gives some headache and buzzing in the ears, but that is only showing that the remedy is having the physiological effect which is essential to the due development of its power of depressing temperature. With these three remedies in our hands, not every case of typhoid requires cold baths; but if the temperature keeps rising, and remains persistently over 104° Fahr., then the bath should be brought to the bedside, carefully raised to the temperature of 70° Fahr., and the patient placed in it for ten minutes. This will cool the surface, but will have, in the first instance,

little effect on the internal temperature, for causes on which we cannot now enter. After the patient has been replaced in bed, however, the internal heat also falls, but too often only for a time, insomuch that in bad cases the baths may require to be repeated every two hours, and as many as 200 to be given in the course of the disease. Thus used there can be no question of the exceedingly great usefulness of cold water baths; the only diversity of opinion seems to be as to the proper time, as indicated by the patient's temperature, when to begin them. But even beyond this there are exceptions to ordinary rules. The effect of the cold water is to arrest or diminish cutaneous circulation, and consequently to produce some degree of internal congestion. If, therefore, there be threatening of intestinal perforation, when the necessary moving of the body might be dangerous, or of intestinal bleeding, when the congestion might give rise to troublesome hemorrhage, the cold water cannot well be employed in this fashion; but it is not to be forgotten that the pulmonary congestion, so apt to be a dangerous concomitant of typhoid, is relieved rather than otherwise by the use of the bath. (Editor of Medical Times and Gazette, p. 10.)

Cold Baths.—It is necessary to combat the excess of temperature during the whole course of the disease; that is to say, *to prevent exacerbations and maintain remissions*. The first point in the treatment lies in its employment from the outset of the disease, for it is certain that refrigeration is more efficacious in preventing impending lesions than in curing them when the high temperature has produced a dissolution of the blood, or involved important organs. The phrase “from the outset” is to be understood as referring to the moment when the diagnosis of enteric fever is certain; or as soon as the physician is called. Precious time should not be lost in using drugs, and only resorting to cold baths when the case appears urgent. Further, there is no harm in treating by this method febrile states that have an analogy to enteric; their evolution, in fact, is more rapid under this treatment than by the use of medicines. There is no means of knowing that a mild case of enteric fever may not suddenly assume a most serious aspect. It is then too late. The following is a description of Brand's method of using the cold bath. He prefers the plunge bath, which is the most suitable, its action not depending on the zeal of the nurse. The water should be at the same temperature throughout, and it should not be a tepid bath gradually cooled, the shock in the former case being regarded as beneficial. The instructions to the nurse are as follows:—The nurse will, every three hours, take the

rectal temperature of the patient, and give him a bath at 20° C., of 15 minutes' duration, night and day, until the thermometer placed in the rectum for five minutes does not register 38.5° C. The patient is taken to the bath, his night dress removed, and he is plunged *up to the neck* in the water, at 20° C., while the head is sprayed with water at 6° to 8°—an important detail, especially where the patient presents cerebral symptoms. After the nervous symptoms have been allayed, the spray may be given at the same temperature as the bath. That affusion having lasted one or two minutes, the nurse rubs the limbs of the patient for three or four minutes; then he is left at rest. His breathing may become difficult, and his teeth may chatter, but he must remain in the bath for 15 minutes. When he is about to be removed from the bath the affusion is to be repeated. He should be kept at least 15 minutes in the bath, even should the shivering set in from the commencement; and longer should the shivering be late of appearing. He is then removed, his night dress put on, without drying him, a sheet is thrown over his feet; his mattress should be hard enough not to yield to the weight of the body, which should be covered by a sheet in summer (also by a light linen cover in winter). A little weak tepid soup is now administered, along with a mouthful of old wine, and he is left alone to rally from his shivering, which lasts 15 to 20 minutes, sometimes for even an hour. Fluid nourishment should be given regularly, and always tepid—mouthfuls of iced water should be taken from time to time. If the patient is very weak, a spoonful of old wine may be administered before the bath. (M. F. Glénard, p. 12.)

TYPHUS FEVER.—*The Eruption.*—The eruption usually appears as small discrete spots, slightly elevated, of a dingy red colour and fading on pressure. In a short time the spots cease to be elevated, and fade less completely on pressure, and a purple mottling appears in the interjacent portions of skin. At a still later period—say on the eighth, ninth or tenth day, the spots become entirely petechial, not being at all affected by pressure. Sometimes the eruption which first appears is more discrete and more decidedly elevated, so as to bear for the time a close resemblance to the typhoid rash, but when the general mottling appears there can be no difficulty in recognising the nature of the eruption. (Dr. T. B. Peacock, p. 25.)

AFFECTIONS OF THE NERVOUS SYSTEM.

CHOREA.—I have found Jaccoud's plan extraordinarily successful in several cases; it consists of spraying the skin over the whole length of the spine with ether twice daily. This, along with arsenic internally, have given me solid results, such as have been obtained by no other remedy. Cod-liver oil and iron, however, are very useful in debilitated subjects. I have a complete distrust in chloral, and zinc I have found a very unreliable remedy. In the milder and more chronic type of chorea, where the nervous system is not too much disorganized and protracted, the shower bath is well worthy of being called a sheet anchor. It gives that preliminary bracing to the nervous and moral tone, without which we may fail to get a leverage for other treatment. (Dr. F. E. Anstie, p. 74.)

ELECTRICITY FOR RELIEF OF PAIN.—Electricity will often cure pain, and much more often will alleviate it; and as an alleviator of pain it is not open, if properly used, to the objections which are applicable to most other anodyne therapeutic applications. By teaching a patient what are the anodyne effects of morphia, chloral, Indian hemp, or alcohol, we may destroy his sufferings only at the expense of his physical and moral well-being. Again, the after-effects of anodyne drugs are often only less disagreeable than the symptoms which they have removed. Electricity is not open to these objections, for, if it does no good, it does not, I believe, do any harm; and for this reason, if for no other, it deserves every trial as an anodyne. All three forms of electricity are employed for the relief of pain, but the most generally useful for such purposes is undoubtedly galvanism. There are many cases of neuralgia on record which have resisted every known method of treatment, but have yielded after a few applications of the constant current. In employing the current for this purpose, I think the true *locus morbi*, if it can be made out, should be included between the poles. If there is tenderness over any of the vertebræ corresponding with the part of the cord from which the painful nerves arise, one of the rheophores should be placed at or a little above this spot. The other rheophore should be applied seriatim to the various "painful spots" along the course of the nerve. The pain under the left breast so common in anæmic women is not unfrequently relieved by electricity, as is also ovarian neuralgia, and some forms of lumbago. Rheumatic pains localised in certain muscles, most commonly the deltoid, and sciatica, are also frequently rapidly relieved, by electricity. (Dr. G. V. Poore, p. 45.)

FACIAL NEURALGIA.—*Gelseminum*.—*Gelseminum* administered in doses of fifteen minims of the tincture every six hours in an ounce of dill water will rarely fail to give decided and lasting relief in cases of facial neuralgia so often seen amongst badly nourished women, if unconnected with any evident local inflammatory changes. The pain does not usually disappear until after the third or fourth dose. (Dr. J. Sawyer, Dr. E. Mackey, pp. 43, 44.)

HYSTERICAL PARALYSIS.—Hysterical paralysis is especially susceptible to electrical treatment, and with it we occasionally work apparent miracles. When a patient who has no will to move her muscles sees that her muscles can be made to move in spite of her will, it is no wonder that such a discovery should exercise a wholesome moral influence over her mental condition. (Dr. G. V. Poore, p. 56,)

NEURALGIA.—*The Chloro-Phosphide of Arsenic*.—The chlorophosphide of arsenic is an excellent preparation of phosphorus for internal administration in cases of neuralgia. It is also more a general tonic than any other preparation of phosphorus. It is prepared by bringing phosphorus and arsenic together in a finely divided state in the presence of hydrochloric acid. This is an operation requiring great caution in manipulation. The dose is from 15 to 20 minims three times a day, after meals. (Dr. C. H. F. Routh, p. 65.)

NITRITE OF AMYL.—Nitrite of amyl, when inhaled during a state of unconsciousness, has a specific action upon the motor centre of the mouth, and calls into action, by preference, the muscles of the lips and lower jaw. The mode in which it performs this action, whether reflexly or through the agency of the vaso-motor apparatus, is as yet only a subject of speculation. The fact that the movements which it evokes are consentaneous with the appearance of flushing in the face, gives probability to the latter hypothesis; while, on the other hand, one observation, that when administered hypodermically the nitrite failed to induce yawning in a case in which it had induced it when inhaled, seems favourable to the other view. (Dr. J. C. Browne, p. 87.)

Mr. Smith's Inhaler for Nitrite of Amyl.—In order to use nitrite of amyl successfully an inhaler is necessary, as some cases of angina pectoris demand its very rapid and thorough administration, and moreover it is an expensive remedy, and the usual primitive method of exhibiting it is wasteful. An inhaler admirably adapted for the purpose has been made for Mr. C. J. Smith by Krohne & Sesemann, of Duke Street, Manchester Square. (Mr. C. J. Smith, *Lancet*, June 20, p. 871.)

SCIATICA.—Sciatica is not a rheumatic disease, except in a few rare cases. In the ordinary form of the disease iodide of potassium is useless; but in syphilitic patients its action is often marvellous. In every case it is well to consider the possibility of the patients having had syphilis, perhaps, at some long antecedent period of life. The pressure of a loaded bowel, a gravid uterus, or a tumour, upon the nerve within the pelvis, may produce the disease, and these questions must be carefully considered in the treatment. In cases where there is a conjunction of the gouty with the neurotic temperament the patient should sedulously avoid beer and all saccharine wines, and should be very moderate in his total allowance of food, especially of meat and other distinctly nitrogenous foods. Vichy water may do good in these cases. Most cases of sciatica are embraced in none of the foregoing classes, but are simple neuralgias. In no instance of sciatica ought we to allow the pain to continue very long before putting in action a remedy which has approved itself in the hands of some of the best observers in Europe, of the highest value for sciatica,—I mean the constant battery current, a remedy so powerful (particularly in this form of neuralgia) that, but for the expense and trouble attending its use, it should be employed as the sole treatment in three-fourths of the cases of sciatica. It is absolutely necessary to have a good instrument, such as Weiss's or Stöhrer's constant-current machines. From twenty-five to thirty-five cells will commonly be required, and the best method of application, on the whole, is the following:—The negative pole (the poles are broad moist sponges) is applied as nearly as possible opposite the roots of the nerves which form the sciatic, and the positive pole is applied in succession to the several foci of pain. The poles should be kept continuously applied for about three minutes at each of these situations, and this should be done either once or twice daily. The supreme utility of hypodermic morphia is such that it can rarely be judiciously omitted in the early treatment of sciatica, especially as the continuance of agony has a peculiar shattering effect on the nerve, which leaves it far more liable to pain than before. (Dr. F. E. Anstie, p. 35.)

TETANUS.—*Chloral and Bromide of Potassium*.—In a case of tetanus in a boy fourteen years of age, ten grains of chloral hydrate and twenty grains of bromide of potassium in syrup and water were given every three hours, the case being watched with great care. The next day the same was given every two hours, with the result of procuring four hours' sleep, with diminution of the tetanic spasms. The

case went on satisfactorily, and on the fourteenth day the chloral was discontinued, as its action was so marked, but the bromide was continued in ten-grain doses for a few days longer. The noteworthy feature in the treatment of this case is the quantity of chloral taken by the patient, he having taken 1140 grs. in sixteen days (equal to fully 71 grs. a day) in a most acute attack of tetanus, with the result of the spasms leaving him on May 12th, exactly eighteen days from the date of seizure; while in their place the peculiar action of the medicine showed itself in a variety of ways. All kinds of delusions ensued. (Dr. J. B. Carruthers, p. 92.)

TIC DOULOUREUX.—*Deep Injection of Chloroform.*—Dr. Bartholow (of Ohio, U.S.), recommends as a useful therapeutic expedient the deep injection of chloroform in cases of tic douloureux, by this is meant the injection of chloroform deep into the tissues in the neighbourhood of the affected nerve. In the cases which have been reported, the infra-orbital branch of the nerve has been the seat of the *tic*. In these cases the operation consists in passing the needle under the upper lip in the direction of and near to the infra-orbital foramen, and then injecting from ten to twenty minims of pure chloroform. Very considerable pain is experienced at the moment of the injection, and for a few minutes subsequently, but this presently subsides and is succeeded by a feeling of numbness and of anæsthesia of the parts into which the chloroform diffuses. A puffy swelling quickly forms at the site of the injection, and an induration, which remains for several days, follows. The numbness of the lip and cheek continues for a variable period—for a week or more. Systemic sensations, such as giddiness and sopor, due to the diffusion of the chloroform into the blood, are soon experienced, but these effects may be scarcely perceptible, and are never alarming. Indeed, the results, so far as systemic effects are concerned, may be regarded as absolutely free from danger. (Dr. R. Bartholow, p. 78.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

ANEURISM OF THE ARCH OF THE AORTA.—*Electrolysis.*—In some cases at all events, the want of success has been due in great measure to using it too strong, and that it is much better to employ a weak current, and, if necessary, to continue the operation for a longer period of time. The needles to be used should be sharp and not too coarse, and that portion which penetrates the skin, the walls of the sac, and intervening tissues should be insulated with vulcanite. They should be oiled before being introduced. In the majority of

applied to the brachial artery, so that not a drop of blood is allowed to pass. In the case from which these directions are taken the tumour became consolidated in forty hours. (Mr. W. MacCormack, p. 247.)

In a case of aneurism of the palmar arch, which had spontaneously burst, the following was the plan of treatment adopted. The thumb and fingers were bandaged separately with narrow strips of calico, carried evenly from their extremities upwards toward the hand. These were fixed, with the fingers flexed upon the hand, and firm pressure made by carrying several turns of a calico roller over the compress, and round the hand. Compresses of lint, about four inches in length, were laid along the line of the radial and ulnar arteries, and secured by a roller bound firmly round the wrist and forearm, as far as the bend of the elbow. The hand was placed across the chest, and the limb confined in a sling, the forearm being flexed somewhat beyond a right angle. The patient was kept in bed, and his arm flexed and immovable. The bandages and compress were renewed on the sixth day, and allowed to remain ten days more, when the discharge became purulent. By degrees the suppuration ceased, and the case was permanently cured. The object of the pressure is to check the force of the whole circulation in the limb; not only the current through the larger arterial trunks—the radial, ulnar, and interosseous, in the forearm and at the wrist—but also through the smaller vessels and capillaries in the hand and fingers, until the injury is repaired. This is not done effectually unless the pressure is made equally on the distal side of the palmar arches, beyond the aneurism or other seat of injury. (Mr. E. L. Hussey, p. 242.)

POPLITEAL ANEURISM.—The first thing in a case of popliteal aneurism is to determine the nature and strength of the sac, the amount of coagulum contained in it, and the position of its orifice, and we should base our choice of the method to be first adopted upon these. The choice between the ligature and some form of pressure (including genuflexion amongst the latter) will rest chiefly upon the presence or absence of a thick, well-formed sac, lined with some amount of coagulum; on the judgment of the surgeon as to the relations of the sac to its orifice; on the acute or chronic nature of the symptoms; and on the presence or absence of disease of the heart, arteries, or kidneys. The most promising cases for compression are those with a thick sac and a certain amount of fibrine deposited, without urgent symptoms or rapid advance, and where the characters of the bruit and of the

pulsation point to the conclusion that the aneurism is not fusiform. And in these cases where the presence or other general complications render the surgeon anxious to avoid the risks attending a serious operation, compression has very often succeeded; but many cases of successful ligature of the femoral artery have occurred in patients suffering from cardiac or renal affections, or with aneurisms in other parts. If compression does not succeed at first, how long should it be persevered with? This question can only be answered in very general terms. Considering the distress and disappointment, and sometimes acute suffering, to the patient of prolonged compression; considering, too, that experience up to the present time points to the conclusion that a patient is not more, but less likely to recover from ligature of the femoral if a prolonged application of pressure has previously been made; and if it can be shown that the Hunterian operation is much less dangerous than we have been taught to regard it, judging only from the experience of a former generation, recorded by Norris and other statistical writers,—it would be better to abandon the compression treatment after a careful, but very moderate trial, if it does not seem to be doing good, and then to resort to the ligature before the patient's chance of recovery from that operation has been permanently impaired. As to how long a time is implied by "a very moderate trial," that must be decided by the individual cases, but my own opinion is that more harm than good generally results from protracting the attempt beyond a week. (Prof. Holmes, p. 236.)

PURPURA HEMORRHAGICA.—*Hypodermic Injection of Ergotine in.*—In a severe case of purpura hemorrhagica, occurring as a complication of typhoid fever, every kind of styptic used failed, and the case appeared quite hopeless. Under these circumstances I injected hypodermically one grain of the liquid extract of ergot at a time. After the first injection, the hemorrhage from the nose, the stomach, the bowels, and the bladder diminished greatly, while fewer new purpuric spots appeared on the body. The second injection completely arrested the hemorrhage. The patient being nourished and stimulated freely her strength rallied, the fever came to a crisis a few days afterwards, and she made a perfect recovery. (Mr. W. L. Lane, p. 252.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

ACUTE TUBERCULOSIS.—Acute miliary tuberculosis often commences with the symptoms of a common cold, and subsequently

so much resembles enteric fever that even with the aid of the thermometer there may be much doubt as to the diagnosis. The duration of the disease and the symptoms in tuberculosis and enteric fever are so much alike that it is probable the one is not unfrequently mistaken for the other, and that the rarer disease is set down as an example of typhoid fever presenting, it may be, supposed anomalous symptoms. In other cases it is apt to be mistaken for capillary bronchitis, or even meningitis. The general febrile condition, delirium, and diarrhoea of acute tuberculosis give a certain resemblance to a case of enteric fever; but the history of cough and pain in the chest, the dyspnoea and lividity of face, with the almost negative information afforded by the physical examination, ought to lead us to a correct conclusion as to the nature of the case. In fact, a livid countenance, dyspnoea, and greatly quickened respirations, with feeble breath-sounds, or comparatively few rhonchi, and a high temperature, together with the absence of dulness on percussion and of bronchial breathing, seem to be the most positive signs of the existence of a primary pulmonary tuberculosis. (Dr. J. W. F. S. Shand, p. 107.)

PHTHISIS.—It is quite a mistake to suppose that phthisis is always or even generally a constitutional disease. It is, on the contrary, in a large number of cases entirely local in its origin, having nothing to do with tubercle or any constitutional taint. In many instances, when tubercle is found in the lungs at the post mortem examination, it is an effect and not the cause of the disease. When the formation of tubercle is the primary morbid condition, much of the subsequent destruction of the lung tissue is the result of the inflammatory lung products to which it gives rise by its irritation. (Dr. F. T. Roberts, p. 96.)

Raw Meat and Phosphate of Lime.—The use of raw meat has been generally found impossible for any length of time, simply because the patient cannot be induced after a certain time to swallow it, or the stomach to tolerate it. The fault rests entirely with the mode of administration. A pound to a pound and a half of fresh beef, deprived of fat, bones, &c., is placed over a quick fire for a few minutes, in order to whiten and harden the external surface only; the piece of meat is then cut into two or three pieces corresponding to the size of the meat-press, and all the juice is extracted by the pressure of the powerful screw. The superficial coction is necessary to overcome the elasticity of the meat, which renders the extraction of the juice a very difficult matter unless more powerful machines be used than the simple one at

present required. A pound and a half of good fresh meat gives a teacupful of juice. The juice should be prepared daily. This juice, having all the physical properties of raw meat, is easily digested, is well tolerated, and, served in the following manner, is always very grateful to the patient. The juice should be mixed with equal parts of tepid broth, made of bones and flavoured with salt and pepper, and to which tapioca, vermicelli, &c., can be added. Care, however, should be taken that the broth is never more than *tepid*, otherwise coagulation takes place, and the desired effect is not obtained. The treatment of the consumptive patient by this method is the following: Early morning: Warm milk (not boiled), with bread and butter, and, if the appetite be good, some fat bacon and eggs. At eleven or twelve o'clock, breakfast, before which a drachm of the syrup of triple phosphate should be taken; during the meal itself, a dose of the muriatic phosphate of lime, and half the daily allowance of the raw meat juice in some broth; the meal should consist, according to appetite and digestive powers, of fish or poultry, or white meats, fresh vegetables, and a few glasses of good alcoholic wine. Dinner at six o'clock on the same principles; broth, with the remainder of the raw meat juice, and, instead of the triple phosphate, a dessert-spoonful of cod-liver oil can be taken with advantage after the meal, if the liver be not enlarged and fatty, and the digestion good. The muriatic solution, or wine of phosphate of lime, should also be taken during the dinner. At night, before retiring to rest, a cupful of warm fresh milk, diluted one-third with Vichy water. No medicines whatsoever, beyond those mentioned, should be administered, unless some special indications or some urgent symptoms claim their use. (Dr. H. Blanc. p. 111.)

Strapping the Chest.—There is no remedy, except the use of cod liver oil, of more real value in the treatment of phthisis than restraining chest movement by strapping the chest with long strips of plaster. I have made an improvement in the apparatus, which diminishes the frequency of the renewal of the plasters and strengthens their grip. The following description contemplates their application to the upper part of the chest. I have principally used emplastrum roborans spread on swans down. The sheet, which is half a yard wide, is to be cut into transverse strips. Each strip is eighteen inches long: the breadth should be about three-quarters of an inch. The plasters should be only very slightly heated. The first strip runs up the back in the space between the spinal column and the posterior border of

the scapula on the affected side, its starting-point being well below the level of the inferior angle of the scapula. It is to be applied gradually and deliberately, every portion being well rubbed in before the next portion is brought into contact with the skin. It is to be carried over the shoulder and down the front of the chest. In rounding the shoulder it is to be pulled tight and held so while it is being, bit by bit, brought into contact with the front of the chest, the chest just at this period being in the act of strong expiration. The next strip, which is horizontal, commences at the spine, crosses the posterior end of the first strip, passes under the axilla and on towards the sternum. It also is to be applied deliberately and with friction; as it is rounding the chest it is to be pulled tight, the patient at the same time making a forced expiration. Other strips are to be applied in a similar manner, vertically and horizontally time about, until it is judged that a proper grasp of the chest has been obtained. I avoid the scapula as much as possible. Some of the horizontal strips should cross the sternum, and some the spine. A large rectangular piece of plaster should now be applied, occupying the interscapular space and reaching down to the last dorsal spine. Another squarish piece is to cover the front and upper part of the chest between the clavicles and mammæ. These, if smoothly applied, secure the ends of the strips from ruffling up, and give additional *points d'appui*. We usually by this treatment obtain immediate and marked diminution of the cough, cessation of pain, relief of dyspnoea, and reduction of temperature; and the patient usually expresses at once a feeling of great comfort. (Dr. J. McCrea, p. 103.)

PHTHISICAL SWEATING.—*Atropia*.—The sulphate of atropia has some power of arresting the distressing sweats of phthisis. The sulphate of atropia is best prescribed in pill, with extract of gentian; watery solutions are not to be depended upon, for they soon spoil by keeping. The first dose should in no instance be larger than one-eightieth of a grain, and guided by the results, the dose may if necessary be increased to one-sixtieth, or even one-fiftieth of a grain; but if this latter quantity be exceeded, well-marked symptoms of poisoning will almost certainly ensue. (Dr. J. B. Williamson, p. 115.)

PLEURITIC EFFUSION, OR EMPYÆMA.—*Tapping the Chest*.—There is much more difficulty in diagnosing a pleuritic effusion than is generally allowed. Dulness on percussion has no distinctive value, and vocal resonance and oëgophony are variable and untrustworthy, and often absent. Local fre-

mitus is the only sign to which we can look for any real help in cases where we have to decide between pleuritic effusion, aneurism, intra-thoracic cancer, or pleural hydatid. It is to be hoped that teachers will beware of speaking too confidently of the ease of distinguishing pleuritic effusions. It is agreed that when effusions are small they are to be left alone unless a small purulent effusion should disturb the general health. In this case it should be removed at once. Larger effusions, whether serous or purulent, should be removed at once, as they threaten both life and lung. The recorded results of the medicinal treatment of such effusions are very bad; while those of operative treatment are good, or, indeed, excellent. This doctrine has been slowly established, but now is fairly acknowledged by all men of experience. (Dr. C. Allbutt, p. 123.)

In cases of empyæma where the pus has made itself another way, we should still make a free artificial opening, otherwise the pus will be evacuated very imperfectly, the retained quantities becoming decomposed, and the lung in great danger of breaking up. For the evacuation of mere serum, a fine trocar inserted anywhere in the lower chest is sufficient, the fluid runs off, and nothing more is needed. For dropsical effusions, then, we require a simple exploring trocar. For inflammatory and sub-inflammatory effusions we require a carbolised trocar or needle, and some apparatus to prevent the entrance of air. One operation, even, in the latter kind, sometimes suffices if the fluid be serous. The loudly praised instrument called the aspirator may be invaluable in many cases, but in tapping of the pleura I have found it cumbrous and even injurious. I have returned to the use of the old trocar with a flexible tube, by means of which we draw off the fluid under water. When the effusion is purulent a free opening should be made at once. I cannot call to mind a single case, in which closed operations succeeded in effecting a cure, and in all my own cases I believe we have had to come to an open operation sooner or later. Nay, more, I am convinced that in three cases the closed operation has done harm. By relieving the intra-thoracic pressure, absorption has become possible, and hectic has set in. The open operation, on the contrary, if so managed as to give a free outlet to the pus, always allays and generally removes hectic. Of late, when once satisfied that pus exists, I have regarded all closed operations as at best a waste of time. Since the introduction of the aspirator I have repeated my trials of the closed operation with no better results. The advantage of the free posterior operation is the perfect drain-

age, to which both dressers and nurses freely testify. The patient must be kept in bed; no pus can putrefy in the thorax; and no injections will be required in uncomplicated cases. The patient must be rigorously confined to bed, not only for the position's sake, but also to avoid chill. (Dr. Clifford Allbutt, p. 124.)

In using the aspirator, it is of the utmost importance to ascertain and ensure beforehand that the instrument is clean and its joints and taps in perfect order. The pointed tube or needle used for puncturing should always in the first place, have a stilette pushed through it to clear away any particle of dried and encrusted matter, which may block up the passage, or afford a nidus of putrefaction or infection which may get into the wound and set up their undesirable processes. The india-rubber tube connecting it with the vacuum cylinder must also be washed in carbolised water or solution of the permanganate of potash, to ensure perfect sweetness; and finally, just before using, the needle should be dipped in carbolised oil. Those who are sceptical as to these precautions should remember the results which sometimes spring from a mere puncture with a foul instrument or splinter. Then the syringe itself should be examined to see that no air can be admitted through the joints. If this be the case, the air will show itself in frothy bubbles as the fluid rushes into the vacuum, and there will be a danger of its getting into the pleural cavity at the next stroke of the piston; this might prove to be a cause of putrefactive changes in the pleural effusion. The admission of air is most likely to happen towards the end of the operation, when the fluid is exhausted or nearly so. In cases of empyæma the use of the aspirator may not suffice, the trocar may be called for, followed by the insertion of a drainage tube. In any case it is better, I think, to be guided in the choice of the site for operation to a great extent by the percussion sounds and stethoscopic indications. If the dulness of the percussion note, and the absence of respiratory sounds be more decided in one place than another, it will be better to choose that part for operating. It is well to be guided also in the choice of a situation by the presence and amount of intercostal bulging, and the absence of the expiratory wave, on principles similar to those which determine the opening of any other abscess as its "pointing" may indicate. And I would hesitate about puncturing even at the most accredited place, unless this sign was in some degree present. Adhesions are indicated mostly by a certain amount of contraction or depression, more visible at the commencement of inspiration

at the intercostal space where it occurs. In all cases an incision, long enough to admit the tip of the finger, should be made through the skin and fibres of the serratus magnus or latissimus dorsi muscles before the pointed tube or trocar is introduced, otherwise the instrument, especially if slender, may even fail to pierce the skin, or may slip over a rib or be drawn aside by the spasmodic contraction of a muscle, and, slanting off obliquely, may fail to enter the thorax, or may puncture it at a part not exactly contemplated. I make it a point to feel clearly, with the finger, the position of the upper border of the under rib, and in stout subjects also to divide some fibres of the external intercostal muscle before puncturing. In all cases where the puncture is not intended to remain open, the skin should be drawn upwards before using the scalpel, so as to form a valvular covering to the puncture when the instrument is withdrawn. Even where the puncture is a very small one, this increases the safeguards against the admission of air, so important in cases where the effusion is serous only. (Mr. J. Wood, p. 116.)

Strapping the Side.—In a case of pleuritic effusion to a considerable amount, the side was firmly strapped with long pieces of plaster covered with two or three layers of strips of bandage steeped in a mixture of gum and chalk. The pain and distress in breathing ceased immediately the application was made, and absorption commenced speedily, so that in a week the fluid had greatly diminished in quantity, and at the end of a fortnight it was entirely removed. (Dr. F. T. Roberts, p. 105.)

PNEUMOTHORAX.—Pneumothorax usually results from the giving way of a tubercular cavity in the lung, and may be limited or not, according as the point of escape is bounded or shut off from the rest of the pleural cavity by adhesions. In either case the best plan of treatment is to strap the side with pieces of some firm plaster, such as emp. roborans—in the more extensive cases covering this over with strips of bandage steeped in a mixture of gum and chalk. If this treatment is commenced soon the escape of air is limited by restraining the respiratory movements. Should the pleural sac be much distended, however, the best plan of treatment would be to remove the air from the pleura by means of the aspirateur, and then immediately put on the apparatus as described. (Dr. F. T. Roberts, p. 105.)

WINTER COUGH AND BRONCHITIC ASTHMA.—*Ipecacuanha Spray.*—In this common but obstinate complaint our results have been very striking, although in many of our patients so

bad was the breathing that, on being shown into the out-patients' room, they dropped into a chair, and for minute or so were unable to speak, or only in monosyllables, having no breath for a long sentence. We used the ordinary spray-producer, with ipecacuanha wine pure or variously diluted. On the first application it sometimes excites a paroxysm of coughing, which generally soon subsides, but if it continues a weaker solution should be used. The patient soon becomes accustomed to it, and inhales the spray freely into the lungs. At first a patient inhales less adroitly than he learns to do afterwards, as he is apt to arch his tongue so that it touches the soft palate, and consequently less enters the chest than when the tongue is depressed. The spray may produce dryness or roughness of the throat, with a raw sore sensation beneath the sternum, and sometimes it causes hoarseness; whilst, on the contrary, some hoarse patients recover voice with the first inhalation. As they go on with the inhalation, they feel it getting lower and lower into the chest till many say they can feel it as low as the ensiform cartilage. The dyspnoea is the first symptom relieved. The night after the first application the paroxysmal dyspnoea was often improved, and the patient had a good night's rest, although for months before the sleep was much broken by shortness of breath and coughing. The difficulty of breathing on exertion is also quickly relieved; for often after the first administration the patient walked home much easier than he came to the hospital, and this improvement is continuous, so that in one or two days or a week the patient can walk with very little distress, a marked improvement taking place immediately after each inhalation; and although after some hours the breathing may again grow a little worse, yet some permanent improvement is gained, unless the patient catches a fresh cold. We have heard patients say that in a week's time they could walk two miles with less distress of breathing than they could walk a hundred yards before the spray was employed. In some instances two or three days' daily spraying is required before any noticeable improvement takes place, this comparatively slow effect being sometimes due to awkward inhalation, so that but little ipecacuanha passes into the bronchial tubes. The effect on the cough and expectoration is also very marked, these both greatly decreasing in a few days, though the improvement in these respects is rather slower than in the case of the breathing. Sometimes for the first few days the expectoration is rather increased. It speedily alters in character, so that it is expelled much more readily, and thus the cough becomes easier, even before the expectoration diminishes. Treated in this

way the patient is soon enabled to lie down at night with his head lower, and in a week or ten days, and sometimes earlier, can do with only one pillow. This improvement occurs in spite of fogs, damp, or east winds—nay, even whilst the weather gets daily worse, and when the patient is exposed to it the chief part of the day. All these patients came daily to the hospital. Of course it is much better to keep the patient in a warm room. (Dr. S. Ringer and Mr. W. Murrell, p. 137.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

ALCOHOL.—What amount of alcohol increases appetite, when should it be given, and in what form? With regard to the *amount*, I should say that, if I may judge from my own experience on healthy men and on patients, the quantity which should be given for this particular purpose of increasing appetite should not exceed one fluid ounce of absolute alcohol in a day, and I think half an ounce is often enough. If this does not improve appetite, a larger quantity will not do so. I am quite sure we cannot force appetite by increasing the amount of alcohol, but, on the contrary, shall run the risk of lessening it. Alcohol has sometimes to be given in large quantities to urge on a failing heart or to blunt sensation; but this is another thing. In such cases a paramount necessity compels us to disregard the question of appetite. But when, as in most cases, our object is to get more nourishment into the body, then we should keep, I believe, to small doses, and if the medicine fails, try something else. I must add to this, however, by affirming what I am quite sure of by observation, that there are persons whose appetites and digestions are injured by even small quantities of alcohol, and who digest much more and eat much more when water only is taken. The healthy digestion of children is, I think, always injured. (Dr. E. A. Parkes, p. 162.)

DIETETIC TREATMENT OF DISEASE.—A proper admixture of different articles of diet is necessary to preserve healthy digestion. For instance, if only dried meat and bread is eaten, without vegetables, as has been tried by soldiers in order to save the transport of bulky articles, in a few days indigestion, with depression of spirits and an eruption of little pimples, results. The treatment of “irritable gastric dyspepsia” by mild farinaceous and vegetable food is well known, but the rapidity of cure of these cases seems to me to show that the best way would be not to give vegetables

at all, or any nitrogenous food, but for two or three days to let only starches and fats in some digestible form, and without salt, enter the stomach. I found these men could take from two to three ounces of butter in the shape of arrowroot biscuits, and also spread upon these biscuits, without producing any of the dreaded "bilious" effects usually attributed to fatty foods. I have noticed two conditions to be produced by a purely non-nitrogenous diet, which may be useful in the treatment of disease. Such a diet lessens in a very great degree the acidity of the urine. We know that the free urinary acidity is usually attempted to be lessened in two ways: first, by giving alkaline medicines or drugs which become alkaline in the body, as the acetates or citrates; and, secondly, by giving fresh fruits and vegetables, which contain similar salts. Now, in both these plans, it has appeared to me that the acid is merely neutralised, and that its formation is not prevented. On the contrary, I have formerly given some evidence to show that the taking of alkalies may really increase the acid-building processes in the body. What these two plans do, I take it, is to neutralise a urine unduly acid, and thereby render it less irritating to the urinary surfaces; and this is a great service, only that it is not all that is sometimes required. A non-nitrogenous and non-saline diet lessens the formation of acid, and to such an amount that it may be made of use in the treatment of cases of lithiasis. The mode in which I gave the non-nitrogenous diet was as follows:—Butter was melted in a jug placed in a water-bath, and the liquid oil was poured off. Arrowroot cakes were made with a portion of this butter, and a little sugar was added. A good cook can thus make very agreeable cakes, which should not be in the least tough. These arrowroot cakes were eaten by my patients with great relish when buttered; they also took arrowroot jelly sweetened. The healthy men on whom I experimented ate as much of this food as they liked; they took on an average ten ounces of arrowroot, six ounces of sugar, and two ounces and one-third of butter, in each twenty-four hours. This quite satisfied hunger, and maintained weight; and all four men on whom I made these trials liked this diet, and felt perfectly well on it. The longest time I kept a man on it was five days, and he did a hard day's work on the fifth day. If it is wished to add a little nitrogen to suit a diet, but not to give meat or bread, it is best done by substituting ground rice for some of the arrowroot and sugar; one ounce of rice contains three grains and a half of nitrogen. If more nitrogen is desired, then it may be given in the form of eggs (yolk and white); an

egg of two ounces in weight contains nine grains and one-third of nitrogen. The influence of this diet of arrowroot and butter oil on the free acidity of the urine is very interesting. It shows us that something still remains to be solved, even after the important observations of Bence Jones, William Roberts, and J. Vogel. It is clear that the free acidity of the urine is unconnected with the changes in the body of the starchy and oleaginous aliments. The lactic acid formed by the one does not, we know, appear in the urine, and the fatty acids coming from the other appear also to have no effect. (Dr. E. A. Parkes, p. 154.)

ENLARGEMENTS OF THE LIVER AND SPLEEN AND GLANDULAR SWELLINGS.—*Dispersion by Deep Puncture.*—Enlargements of the liver often disappear rapidly after repeated plunges of an ordinary hydrocele trocar when seeking unsuccessfully for suspected abscess, and without the occurrence of any inflammatory symptoms. The native hakims of the East have been accustomed to puncture the liver and spleen with long sharp stilets, in order to bring about the absorption of those organs so common in hot countries. Other enlargements besides those of liver and spleen may be made to disappear by puncture. Nothing is more tedious than those chronic glandular swellings which, in strumous subjects, often in hot countries follow upon trifling causes, such as angry mosquito bites, riding a rough bucking horse, over-exertion, or a strain in cricketing and so forth. I have seen an officer laid up for many months, and ultimately invalided with a large mass of indurated enlarged glands occupying the whole inguinal region, and resisting all the recognised routine of treatment. Accidents showed me that deep puncture of such masses with a common lancet held at right angles to the swelling, and pushed down to its bottom, will often cause absorption to set in and proceed rapidly. The first case in which this occurred to me was one of a mercantile gentleman, disabled by a mass of swollen inguinal glands, hard as a board almost, and resisting all treatment. This patient's loss of time at office was a very serious matter to him, and, influenced by his despairing impatience, I plunged a lancet perpendicularly into the mass as far as it would reach. The point came out tinged with matter, and hard pressure brought up a little cheesy, ill-formed pus, but no discharge whatever followed, and absorption set in and proceeded rapidly. (Deputy Inspector-General Cameron, p. 308.)

Exploration of the Liver by Puncture.—Those who have never witnessed hepatic explorations are slow to believe in their safety and good effects. In suspected abscess, the enlarged

liver may be punctured again and again to search for the abscess, and no inflammatory or other consequences will result. Even provided no abscess is met with, the best effects possible may be expected from the operation, as the liver, frequently, rapidly diminishes in size after it. If abscess is met with, it is not necessary to leave the canula in, and in many cases rapid recovery follows. It is an unsuccessful plan of treatment to wait for pointing of an hepatic abscess, as the patient often dies exhausted before this takes place, or succumbs earlier from purulent absorption. (Deputy Inspector-General Cameron, p. 310.)

FUNCTION OF THE LIVER.—In the normal state the liver is always doing two things: it is *forming* new bile, and it is *excreting* old bile which it has received from the intestine by means of the portal vessels. When a biliary fistula is made and the bile is drawn away as fast as it is secreted, none gets into the intestine, and therefore no old bile reaches the liver; consequently, the quantity collected represents only the new bile formed in the liver, and is of course much less than that which would normally pass through the ductus choledochus into the intestine. If all the bile were absorbed there would be no need for the liver to go on forming it, but this is not the case, for only a part of it is reabsorbed, and the remainder is decomposed and excreted with the fæces. (Dr. T. L. Brunton, p. 167.)

HEMORRHOIDS.—The old operation of tying an internal hemorrhoid is an excellent one, but it has the disadvantage of being very painful. The operation may be performed with the elastic ligature. The pile is seized with a pair of forceps which closes with a spring or a screw and terminates in a ring. The elastic ligature is carried with the point of the finger several times round the end of the forceps and tied. The protruding pile or piles are then returned into the rectum. They slough off in the same manner as if tied with a silk ligature, but there is this additional security—namely, that the ligature always remains tight. If the pile be large, and a ligature is applied, after it has eaten its way partly through it becomes loose, and should the pile not be completely dead the circulation in it may be in part restored, and there is some danger of the products of the decomposing mass becoming absorbed. It is astonishing with what rapidity the elastic ligature will cut its way through. The elastic ligature, if used for this or for other purposes, should be solid. If tubular, when stretched it flattens against the parts, and, consequently, does not cut through them so easily. By far the best and safest way, according to my experience,

of removing a pile, is to grasp it at its base with a clamp made in the shape of a pair of scissors, curved on the flat, and with the points turned inward; then to cut the pile off with a pair of scissors of the same shape, and to sear the cut surface with an iron after it has passed from a dull-red to a black heat. I much prefer the heated iron to the galvanic cautery. When these fail the nitric acid is an excellent remedy. The best way of applying the acid, according to my experience, is to introduce a small speculum into the bowel, such as was first described by Sir William Fergusson. It has an opening on its side or near its extremity. It may be moved about until the pile projects into the opening. The pile is now wiped dry, and the acid applied on a glass rod. Several successive applications may be made in this way, but during the process the speculum should be held perfectly still. When the acid has produced the effect desired, some chalk-and-water should be freely used, so as to neutralise any remaining acid, and then the speculum may be removed. Performed in this way the operation is perfectly safe, and, as far as the acid is concerned, painless. (Mr. Henry Lee, p. 256.)

Injection of Tincture of Perchloride of Iron.—Internal hemorrhoids are vascular growths, resembling nævus in children, or erectile tissue in adults. They may be entirely destroyed by injecting into each hemorrhoidal tumour about twenty minims of the ordinary tincture of perchloride of iron by means of a hypodermic syringe. To enable this to be done the piles must be forced down so as to be extruded. In a case thus treated, on examination four weeks afterwards, no trace of the piles could be discovered, except three nodules of cuticle, each the size of a shrivelled currant. (Mr. W. Colles, p. 269.)

Linear Cauterisation of the Anus.—The patient, whose rectum has been emptied in the morning by means of an injection, ought to be chloroformed; but if he prefer to remain awake, it is of little importance, as the operation only lasts some seconds. He is laid on the edge of a bed, with one leg extended, and the other bent as if he were going to be operated on for fistula. The assistant raises the disengaged buttock, the surgeon paints the anus and the surrounding parts largely with collodion, whilst an assistant, by means of bellows, drives off the fumes of the ether, which are sure to catch fire when a highly heated cauteriser is brought near them. During these preparations, two knife-shaped cauterisers have been placed in a small furnace, full of charcoal or burning wood. The blades of these cauterisers should be

two centimètres long and one wide; the tip and edge should be blunt, as in ordinary cauterisers, but the back should be four or five millimètres thick, so as to hold enough heat. The surgeon takes one of these cauterisers when it is white hot, and introduces it about one centimètre into the anus, bearing with the shoulder of the instrument rather more on the cutaneous than on the mucous orifice, and makes four cauterisation lines, before, behind, on the right and on the left. The operation is terminated when it has lasted five or six seconds. Simple water dressings should then be applied. The tumour soon ceases to be painful, and it is at last completely and spontaneously retracted. (Dr. Woillemier, p. 262.)

Hemorrhoids, Prolapsus Recti, and Fistula in Ano.—The Galvanic Cautery.—Cases of hemorrhoids and prolapsus recti can be treated with greater facility, confidence, and success, with the galvanic cautery than by any other plan of treatment; and fistula in ano can be divided without any loss of blood. The operation for the removal of hemorrhoids, or the cure of prolapsus recti, with the galvanic cautery, differs in no respect from that usually performed with the common cautery. The bowel to be treated is brought well down and into view either by means of the patient who is made to strain over a pan of hot water, or by an enema of warm water. The different portions of the bowel to be removed are then seized and clamped in vertical pieces, radiating from the anus, each clamped portion, or rather the upper half of each portion, being then cut off with scissors. The parts are then made ready for the application of the cautery—the porcelain cautery heated to a red heat. This is to be rubbed over the surface of the projecting mass till it is burned down to the level of the clamp and turned into a dry eschar; each piece is to be dealt with in order, and each clamp removed as its segment is cauterised, the whole being carefully returned and pressed into the rectum when the operation has been completed. When the cautery is efficiently applied no bleeding follows this operation, and very little pain. The pain induced after the galvanic cautery is far less than that which follows the actual cautery. During the application of the cautery the soft parts should be well protected by strips of wet lint or ivory spatulas. In cases of piles as represented by the growth of small vascular strawberry-looking tumours, I have been content with the direct application of the cautery to the growth, having exposed it well by means of a speculum; and I have found this practice a good one. In other cases, again, of prolapsus recti, I have simply scored obliquely in

lines the surface of the mucous membrane of the prolapsed bowel, and returned it; and this treatment has been followed by good success, more particularly when employed in the milder forms of the affection. In cases of long standing such treatment is inapplicable. Nothing but the removal of portions of the prolapsed mucous membrane will effect a cure. The operation for the cure of fistula in ano by the galvanic cautery is performed as follows:—The platinum wire, or twist of platinum wire, is first introduced through the fistula, and I generally do this through the groove of the probed director when it has been made to pass through the fistula into the bowel. I then hook, with the finger that is in the rectum, the end of the wire downwards through the anus, and remove the grooved probe, in this way one end of the wire being made to protrude through the rectal orifice of the fistula, out at the anus, and the other through the external orifice of the fistula. The two ends of the wire are then connected with the poles of the charged battery, and the wire heated, the division of the fistula being made either by means of the *écraseur*, or a gentle sawing movement of the wire, or traction upon it. The wound may then be dressed with oiled lint or cotton-wool, and the case treated upon ordinary principles, the wound having simply to heal by granulation. (Mr. T. Bryant, p. 264.)

IODIDE OF POTASSIUM IN THE SYSTEM.—When iodide of potassium is swallowed it is absorbed from the stomach, passes in the blood to the salivary glands, and is excreted by them much more readily than by the kidneys. It is, however, again and again absorbed and excreted, and does not get out of the system unless a purgative is given to cause its expulsion from the bowels before it can be reabsorbed. The same is the case with the other iodides, such as those of lead or iron. (Dr. T. L. Brunton, p. 164.)

LEAD POISONING.—The lead is present in the body in the form of an insoluble compound which it makes with the tissues, but by the administration of iodide of potassium it is rendered soluble. It then finds its way into the circulation, and is excreted by the kidneys and other emunctories. But the iodide of lead is partly excreted by the salivary glands, for M. Malherbe, of Nantes, and Dr. Sieveking have found it in the saliva of persons suffering from lead-poisoning, and who were being treated by iodide of potassium. The lead salt being swallowed with the saliva, is again reabsorbed, and thus the cure is comparatively slow when patients are treated with iodide of potassium alone. I frequently see patients suffering from lead-poisoning brought on by working

in white lead, and for some time I have been accustomed to treat them with five grains of iodide of potassium three times a day, and a sufficient quantity of sulphate of magnesia or other purgative either thrice or once a day, to keep the bowels very freely open, and cause the expulsion of the lead from the alimentary canal as quickly as it is secreted into it. (Dr. T. L. Brunton, p. 164.)

OBSTINATE VOMITING.—*Enemata of Bromide of Potassium.*—Dr. Girabetti has obtained the very best results from the administration of enemata of bromide of potassium, in doses of from one-half to two drachms, in cases of obstinate vomiting, attending the pregnant state. The same drug, also administered in enemata, has been very successful in the hands of Dr. Laborde, of Paris, in obstinate vomiting connected with disease of the stomach, liver, and intestines. (Lancet, May 30, p. 770.)

PARENCHYMATOUS INJECTION OF CARBOLIC ACID.—Hüter strongly recommends the injection of a two per cent. solution of pure carbolic acid in water as the most active antiphlogistic remedy for all forms of parenchymatous inflammation. He makes the injection in the position where the lymphatics bring the acid in direct contact with the inflamed tissue. The result of such parenchymatous carbolic injection he describes, from experience, as being miraculous; and he says there are no material impediments to its use in organs such as the lungs, spleen, &c., but he has not as yet practised it in these viscera. (Centralblatt, Jan. 24.)

ROUND WORMS.—The best way to prescribe santonine is in a mixture with syrup, so that one grain of santonine is taken three times a day. A case is related in which no less than sixty-two worms were passed under the use of this medicine. (Dr. T. E. Clark, p. 164.)

SEA-SICKNESS.—When a ship is rolling violently let any one watch a decanter of water on the cabin table. The water swashes violently from side to side. The same thing precisely is taking place in all the liquids of the body, and it is this varying pressure on the brain, liver, &c., which produces sea-sickness. The following hints for the prevention of the affection are useful:—The stomach should be absolutely empty before going on board, for I have noticed that sick people generally throw up basinfuls of food on the Channel steamers; but, to avoid exhaustion, a good meal should be taken three, four, or five hours before, according to the nature of the food. Meat requires four, in some five, hours for complete digestion. Then one or two hours before em-

barking, some very strong coffee, tea, or spirits and water, should be taken, without milk or other food. This is to tonify the nervous system, and yet to secure emptiness of the stomach, fluids being usually absorbed in less than an hour. Once on board, repose should be enjoined—the recumbent position is best,—and nothing whatever, solid or fluid, should be taken for twelve hours or more, even then very little. (Dr. J. Henry Bennet, *Lancet*, Oct. 10, p. 811.)

TAPEWORM.—*A Rapid Cure for.*—A. J. Schafish, of Washington, says that in treating some cases of tapeworm, he employed no preliminary provisions beyond forbidding the patient to take any breakfast the day on which it is intended to remove the worm, and giving him a large dose of Rochelle salts the preceding night. At ten o'clock in the morning, he had the following at one dose:—*R.* Bark of pomegranate root, $\frac{1}{2}$ ounce; pumpkin-seed, $\frac{1}{2}$ drachm; ethereal extract of male fern, 1 drachm; powdered ergot, $\frac{1}{2}$ drachm; powdered gum arabic, 2 drachms; croton-oil, 2 drops. The pomegranate-bark and pumpkin-seed were thoroughly bruised, and with the ergot, boiled in eight ounces of water for fifteen minutes, then strained through a coarse cloth. The croton-oil was first well rubbed up with the acacia and extract of male fern, and then formed into an emulsion with the decoction. In each case, the worm was expelled alive and entire within two hours. No unpleasant effects followed. In each case, the worm was passed with the head firmly fastened to the side of its body at about the widest part, from which it was with difficulty removed; and the worm was twisted and doubled into various knots. (*Brit. Med. Journal*, May 16, p. 651.)

AFFECTIONS OF THE URINARY ORGANS.

ALBUMEN IN URINE.—*Test for.*—The most accurate test for albumen is the following: add a little concentrated nitric acid to the urine, which should be contained in a test tube or champagne glass. The acid should trickle down the side of the vessel. Admixture of the fluids does not instantly take place. The urine floats on the surface; the acid is underneath. Usually at the point of contact an intensely red, violet, or blue ring forms—the reaction of uroxanthine. Should the colour be green, bile pigment may be present; but if albumen, a circular and well-defined turbidity succeeds, and the flaky material gradually sinks to the bottom. Should the urine be also rich in urates, a turbidity may also be thus formed; but then the albuminous ring is lower than the one due to the urates, and separated from it by a clear ring, and the latter gradually vanishes towards

the surface, while the former subsides. The application of heat however dispels all doubt. (Medical Press and Circular, Aug. 19, p. 149.)

BLOOD IN URINE.—*The Guaiacum Test for.*—The following very simple test for blood in urine is given by Professor Almen:—A few cubic centimeters of tincture of guaiacum are mixed with an equal volume of oil of turpentine, in a test tube, and shaken till an emulsion is formed. The urine to be tested is then carefully added, so that it may sink to the bottom. When the emulsion and urine come into contact the guaiacum resin separates, and falls as a fine white, dirty yellow, or green precipitate. If blood is present in the urine, the resin will have a more or less intense blue colour, often almost indigo. In normal urine, or that containing albumen or pus, this blue colour does not appear, and thus proves the absence of blood. (Med. and Surg. Reporter.)

COPAIBA RESIN AS A DIURETIC.—In cases requiring free action of the kidneys in order to remove a dropsical or inflammatory effusion there is not any diuretic so satisfactory as the resin of copaiba. Two grains may be given at a dose. (Dr. Moxon, p. 323.)

DIABETES, &c.—*The Action of Valerian.*—The action of valerian is to spare waste of tissue, and this is proved by the fact that if given in cases where there is excessive excretion of urea, as diabetes insipidus, it diminishes the amount. After eight grammes (120 grains) of the extract of valerian have been taken the urea excreted may fall to 600 grains per diem; and there is no diminution in the amount of urine observed until the urea has fallen below its normal quantity, and then only three or four pints may be excreted in twenty-fours. Certain Indians of Lower California and Mexico are accustomed to go through a course of it for a month before they enter upon a severe expedition, so that they may be better able to bear fatigue. The extract should be given in frequent small doses, and gradually increased in amount, until in some cases a dose of thirty grammes (nearly an ounce) in the twenty-four hours has been reached. (M. Bouchard, p. 171.)

ENLARGED PROSTATE.—In the New York Medical Journal for July, Dr. Van Buren contributes a highly interesting paper on this subject, the purport of which is to recommend that cases of enlarged prostate should, from the beginning, be treated by catheterism employed by the patient when he has been sufficiently taught. The catheter should be passed from four to five times a day, and the bladder may have to be washed out; but the patient will be a great gainer even as regards time saved, and especially in absence of suffering

and prevention of retention. The object is to entirely supersede the efforts made by the bladder to overcome the obstruction, and to prevent the consequent suffering and irritation. The patient, however reluctant to adopt the practice at first, soon is able to follow it, and his life is not only much prolonged, but is passed in comparative comfort. (Medical Times and Gazette, Aug. 29, p. 232.)

ESTIMATION OF THE NITROGENOUS CONTENTS OF URINE.—

In some cases it is of importance that we should be able to estimate the nitrogen contained in the urine in uric acid, creatine, creatinine, and other bodies. This may be done in the following manner:—Take a small tube of hard glass, about five inches long and three-eighths of an inch internal diameter; one end of this is drawn out before the blowpipe and bent so as to form an angle of about 45°. The tube is now supported in an inclined position and one cubic centimetre of the urine, about sixteen minims in bulk, very accurately measured into it by means of a carefully graduated pipette. A drop of sulphuric acid is then added and the liquid evaporated in the tube at a gentle heat, a current of air produced by an aspirator being made to pass over the surface of the liquid. When this small quantity of urine has evaporated nearly to dryness, the fine end of the tube is hermetically sealed, a quantity of “soda-lime” is then introduced, and the mouth closed by a good cork carrying a V-tube containing a very little diluted hydrochloric acid. A strong heat is now applied to the mixture of urine residue and soda-lime, and the ammonia resulting from the decomposition of the nitrogenised organic matters driven off. All the nitrogen present in the cubic centimetre of urine is thus obtained in the form of ammonia, the latter being wholly condensed in the acid contained in the V-tube. At the termination of the operation the acid is washed out of the V-tube into a glass cylinder, neutralised with pure potash diluted up to a known bulk, and then treated with “Nessler solution,” the well-known and extremely delicate colour-test for ammonia. The depth of the brown tint produced by the reagent measures the proportion of ammonia, and therefore of total nitrogen, in the quantity of urine taken for experiment. The value of the result of the colour test is very expeditiously obtained by having at hand a number of standard solutions of varying depths of tint for comparison. (Prof. Emerson Reynolds, p. 170.)

HYDROCELE. — *Injection of Carbolic Acid.* — Of the various injecting fluids proposed for the radical cure of hydrocele, tincture of iodine is most generally employed at the present

day; but it often fails to produce the inflammation requisite for the obliteration of the sac, and occasionally induces too much inflammatory action. Dr. Levis, of the Pennsylvanian Hospital, prefers carbolic acid, and in a case of hydrocele, complicated with inguinal hernia, used as an injection, one drachm of a mixture of equal parts of carbolic acid and glycerine. The injection of carbolic acid seems, from other cases to which it has been applied, to fulfil the conditions most admirably, producing sufficient inflammatory action to secure adhesion of the walls of the sac, and giving little or no pain to the patient, either at the time of its introduction or subsequently. This freedom from pain is probably due to the local anæsthetic effect of the carbolic acid, for it is well known that if this article be placed upon the skin, the surface can be scarified with a knife without pain. (Philadelphia Med. Times.)

INCONTINENCE OF URINE.—*Hydrate of Chloral*.—Dr. E. Vecchiotti relates five cases of this malady occurring in his own practice. All had been treated unsuccessfully; and all the patients recovered under the use of rather less than half a gramme (about $7\frac{1}{2}$ grains) of hydrate of chloral, given in a little water in the evening; abstinence from drinks being at the same time enjoined. The effect was rapid, and in most cases permanent after taking the first dose. He explains the action of the chloral by supposing that the bladder is under the influence of two sets of nerves, one connected with the cerebro-spinal system and the other with the sympathetic; that incontinence of urine depends on irritation of the bladder at the same time that the sphincter is withdrawn from the action of the will; and that the chloral reduces the exaggerated excitability of the organ. (Brit. Med. Journal, April 25, p. 550.)

LITHOTOMY.—*Landing Net for Removing the Stone*.—The stone is often crushed during removal by the forceps, it is more safely removed entire by means of a net. The net (of silk or canvas, according to the size of the stones) is made to slide on a curved wire, bent on the principle of midwifery forceps. After the cut has been made into the bladder, the net is introduced through the wound to the side of the stone or fragments; and, by gently tickling the stone and manipulating the net, the stone or fragments (coaxed in) are caught on withdrawal. These stones *plus* silk are the occupants of the perineal wound instead of stone *plus* forceps or scoop. (Mr. R. Davy, p. 277.)

STRICTURE.—*Mr. Teevan's New Urethrotome*.—A urethrotome ought to divide from before backwards, for if the stricture

be capacious enough to permit an instrument to pass which cuts from behind forwards the indication for any operation is doubtful. Five years ago a great advance was made in the construction of the urethrotome by Professor Gouley, of New York, who *tunneled the vesical end of his instrument, so that it could be slid over and along the finest bougie already passed*. So far as I can ascertain, no English surgeon had ever heard of such a thing as a tunneled catheter or urethrotome until it was sent to him from the other side of the Atlantic, and I consider that Professor Gouley's invention marked an era in the surgery of the urinary organs. He, for instance, has converted that dumb and consequently unsafe instrument which is called "Syme's staff" into *a conductor which tells the surgeon where it has gone to*, so that external urethrotomy can now be performed with a certainty hitherto unattainable, and also extended to desperate cases which were formerly beyond its reach. In the Lancet for July 5th, 1873, I gave a description of a catheter with a conducting bougie founded on Professor Gouley's principle, but differing in some respects from his; and I shortly afterwards conceived the idea of adapting the vesical third of the instrument to the urethrotome, in order to endow it with safety and a more extended sphere of usefulness. The instrument is composed of a very slender olivary whalebone bougie (see woodcut, B), not so large as a No. $\frac{1}{4}$, which is first of all introduced, or wriggled I would rather say, into the bladder apparently, when the catheter (A C) with a handle (G) is slid over and along the bougie (B), which emerges from the catheter through the slit which extends from the handle to the point (F). If the bougie have been passed successfully into the bladder, urine will of course flow from the end of the catheter; but if it should not do so, the catheter may yet be in the bladder, for the slit may have permitted some blood or mucus to block up the tube, and it is therefore necessary to pass the bougie through the whole length of the catheter to clear its interior. When it has been proved that the instrument is in the bladder, the knife (E D) may be slid along the catheter firmly and slowly, and the stricture divided. (Mr. W. F. Teevan, p. 272.)

URÆMIC CONVULSIONS.—*Blood-letting in*.—Mr. White, of Sunderland, relates briefly three cases of uræmic convulsions, all of which were relieved by venesection to several ounces from the arm. The first two were middle-aged men with renal disease, and who were attacked with convulsions; both recovered so quickly as to be able to leave the hospital in a few days. The third case was one of scarlatinal albuminuria,

and in this case the convulsive attacks were so rapid and severe that chloroform had to be administered during the bleeding. The patient afterwards fell into a quiet sleep, the convulsions passed away, and he recovered. (Dr. Wm. L. White, *Med. Times and Gazette*, Sept. 19, p. 350.)

FRACTURES, DISLOCATIONS, AND DISEASES OF BONES AND JOINTS.

ANCHYLOSIS OF THE HIP-JOINT.—This operation, as performed by Mr. Adams, is most satisfactory in its results. All that is necessary is a long-bladed tenotome and a narrow-bladed saw with a good handle. The saw is three-eighths of an inch in width, and with a cutting edge an inch and a half in length. Mr. Adams describes his first operation as follows:—I entered the tenotomy-knife a little above the top of the greater trochanter, and carrying it straight down to the neck of the thigh-bone, divided the muscles and opened the capsular ligament freely; withdrawing the knife, I carried the small saw along the track made, preserving this by pressure of the fingers straight down to the bone, and sawed through it from before backwards; the section of the bone was accomplished in five minutes. No hemorrhage followed, and I immediately applied a compress of dry lint retained in position by strips of plaster and a bandage; as soon as the bone was cut through the leg moved freely in all directions, but before it could be brought into straight position it was necessary to divide the tendons of the long head of the rectus and of the adductor longus muscles, and to cut through the tensor vaginæ femoris muscle. The limb was fixed in a straight position and bandaged to a long interrupted Liston's splint; no inflammation whatever followed the operation; no swelling or redness of the skin or any deep suppuration, but the wound healed slowly. (Mr. W. Adams, p. 196.)

BLOODLESS OPERATIONS.—All that is necessary for the performance of a bloodless operation on a limb is about three feet of india-rubber rope half an inch in diameter, and an elastic bandage two inches and a half wide and six yards long. To the ends of the india-rubber rope a couple of hooks should be fastened. Whilst the patient is being chloroformed the limb is to be bandaged from the toes upwards (supposing it to be a lower limb), the bandage being kept on the stretch whilst it is being applied, so that the entire limb is firmly and equally constricted. So soon as the anæsthesia is complete the india-rubber rope is applied close to the upper border of the bandage, being turned around the limb

three or four times, and the ends hooked together, the metal hooks resting on the subjacent rubber, and not pressing upon the soft parts. If the rope be long relatively to the circumference of the limb it is always possible to graduate the constricting force sufficiently, and it is important to know that the amount needful completely to shut off the current of blood is not so considerable as one might *à priori* suppose. Esmarch uses a tube three quarters of an inch in diameter. When it is desirable to distribute the pressure over a larger surface the tube is preferable to the solid rope. If economy of space, be an object, as in an amputation high up in the thigh, then the smaller sized rope possesses the advantage. In operations upon the upper extremity, the male genital organs, or the fingers, a smaller-sized tube or rope should be selected. Before applying the rope an interval of a minute should be allowed for the compression to empty the limb completely of blood. The elastic bandage may now be unrolled. By these simple means it will be found that complete local anæmia is produced. The bandaging has driven, practically, all the blood out of the limb, whilst the circular compression above completely prevents any from gaining admission during the time it remains applied. The limb thus treated appears of waxy pallor, and the temperature becomes greatly reduced. The wound may be plugged before removing the constricting rope, and no bleeding takes place; but after amputation or excision it is impossible thus to secure all the bleeding points, and after securing the principal vessels the elastic rope must be removed before closing the wound. Possibly this method of Esmarch may prove useful in the treatment of certain aneurisms. We know now how readily aneurisms may often be cured in a few hours by the complete arrest of the flow of blood through the sac. For instance, in a popliteal and femoral aneurism we might easily and perfectly arrest the current of blood, and if the limb were previously, to some extent at least, if not completely, emptied of blood, the aneurismal sac would be allowed the opportunity of contracting in size, which would certainly prove a powerful adjunct to the compression in furthering a cure. (Mr. W. MacCormack, p. 246.)

DEPRESSED FRACTURE OF THE SKULL.—It is not necessary to operate in a case of simple fracture of the vault of the skull, with depression of bone but no symptoms of compression, unless the bone is driven in edgeways. Much importance is laid upon the question of whether the fracture be simple or compound in standard works on the subject. I think too much stress has been put upon this point. That it should

exercise a certain influence is, of course, perfectly right, for it is not a light matter under any circumstances to convert a simple into a compound fracture, least so when the skull is concerned. But when, on the other hand, the advantage which may be gained by a successful operation is considered, and the imminent peril in which the patient is placed in certain cases if no operation be undertaken, I think there can be no doubt that the right course to pursue is to cut down upon the bone, and explore. The greatest difference of opinion exists perhaps as to the right treatment to be pursued when there are symptoms of compression, as coma, but without clear evidence of depression. I agree in the main with what Mr. Erichsen has said on this subject—that when such symptoms exist, in all cases of doubt, an incision should be made down to the bone, and the state of the skull examined. Further proceedings will depend almost entirely upon the condition of the skull when thus laid bare. There is one form of fracture in which the rule is constant to trephine at once. This is the punctured fracture, in which the inner table is usually much comminuted. It is of no consequence whether or not there be immediate symptoms of compression; the operation is not performed on account of these, but in order to prevent, if possible, the irritation and inflammation almost sure to be set up by the small and often sharp splinters which are formed. (Mr. W. S. Savory, p. 197.)

DISLOCATION OF THE FOREARM BACKWARDS.—*A Simple Method of Reducing.*—Dr. Alexander Murray writes to the New York Medical Record that he has reduced five cases of the above-mentioned dislocation by the method to be described. Supposing the dislocated arm to be the left. Dr. Murray takes his position at the outside of the dislocated arm, and places the palm of his right hand to the palm of the patient's left, dovetailing his fingers between each of the patient's. In this way, a firm hold is secured for extension. He then places his elbow as a fulcrum and for counter-extension on the forearm in front and against the lower end of the humerus, and by a steady pressure downwards and backwards, and at the same time flexing the forearm towards the shoulder, in a few minutes the luxated bones slip into their natural places. Other dislocations of the elbow can be reduced by the same method. (Med. Press and Circular, Aug. 19, p. 162.)

ELASTIC LIGATURE.—The use of the elastic ligature is much facilitated by the employment of an ingenious instrument devised by Mr. Allingham. It consists of a mounted curved probe with a concealed notch. A blunt or sharp point can

be used with the probe at pleasure. It will be understood at once by reference to the woodcut at (p. 250.)

A Substitute for the Elastic Ligature.—Dr. Hollis describes an instrument which he calls “The Sarcotome,” and which is intended as a substitute for the elastic ligature. The instrument consists essentially in substituting, as a cutting apparatus, a spiral steel spring and a waxed thread for the ordinary caoutchouc tubing, which has hitherto acted as an elastic ligature. (Dr. Hollis, p. 249.)

GUARD FOR THE POINTS OF HARE-LIP PINS.—It is not a good plan to break or cut off the points of pins used in hare-lip or other plastic operations, because if the pin is strong the force necessary is sufficient to disarrange the neatly and accurately adjusted edges of the wound, and also a rough edge is left which renders their withdrawal more difficult and painful. I have lately devised a simple contrivance, which I think of service in such cases. I call it a needle guard. It consists of two little plated tubes, each three-quarters of an inch long, and open at one end. The tubes are tied together with a piece of string elastic, or better still, with a vulcanised india-rubber ring, sufficiently extensible to allow the guard to be easily slipped over the two ends of the needle. The guard does not interfere with the twisted suture or skin and can be at once removed, without the slightest difficulty or dragging, when the needle is about being withdrawn. (Mr. H. J. Tyrrell, p. 255.)

VENEREAL AFFECTIONS.

GONORRHOEA. — *Hydrastin.* — As far as internal treatment is concerned, I merely give in the first stage a saline aperient, to be continued three times daily for four or five days, together with the following injection:—Hydrastin, one drachm; solution of morphia (Magendie’s), two drachms; acacia mucilage to four ounces: to be used three times daily. This I have employed when inflammation ran very high, without even the slightest ill effects, and have used it in every stage of gonorrhœa with the most beneficial results when every other treatment, both internally and locally, had failed, including red sandal oil. But there is one remark I wish to make regarding the use of injections which medical men generally forget, and that is, to tell their patients to micturate previous to its use. Unless this is done, injections in gonorrhœa are useless. Hydrastin is used very much in different parts of the United States, and very successfully. (Mr. J. N. Bredin, *Lancet*, August 15, p. 259.)

GONORRHOEAL OPHTHALMIA OF ONE EYE.—*A Protective Bandage for the Other.*—Some protection for the sound eye is absolutely necessary in cases of gonorrhœal ophthalmia of one eye only. A most efficient one may be made from a small square piece of Macintosh into the centre of which a watch glass is fastened. The macintosh is trimmed to fit the nose and forehead, and is fixed by means of adhesive plaster. (Mr. F. Buller, p. 290.)

PSORIASIS.—Syphilitic psoriasis is easily treated. Small doses of perchloride of mercury, with or without iodide of potassium are the most useful, along with the local application of a 5 per cent. solution of oleate of mercury. Dr. Hughes Bennett used to teach that arsenic internally and pitch externally would cure any case of psoriasis in six weeks. Many cases of psoriasis are intolerant of the action of pitch. In certain persons, and in particular stages of the disorder, the remedy proves too irritant; and, again, arsenic, even in the smallest doses, cannot be borne by some patients. Hence, in many instances, the employment of pitch has to be carried out in a modified form. I have a high opinion of the value of the application in the majority of cases. If pitch ointment is objected to, some of the tarry oils, or carbolic acid, may be substituted; and the odour may in some degree be covered, as Dr. McCall Anderson has shown, by the oil of lemon-grass (*citronella*) or by rosemary. The tarry preparations, and all local remedies indeed, should be only applied to the affected spots, and with as diligent friction as can be borne. The linimentum picis comp. is a favourite application at St. Bartholomew's Hospital; and is composed of equal parts of pitch, soft soap, and rectified spirit of wine. It is best laid on with a painter's brush, or with a piece of flannel. Warm alkaline baths are of much use, not less frequently than twice in the week; and, if the patches will bear it, they may be effectually cleansed and prepared for fresh inunction by friction with soft soap and pumice-stone. It will be found, however, in many cases, that this local treatment is resented, and this is to be looked for in the early stages of the disorder, when the patches are small and hyperæmic, and when fresh spots are still appearing. Irritant treatment is inapplicable to this condition, and, instead, it is proper to employ simple inunction with olive oil, almond oil, or spermaceti ointment. Warm rain-water baths and a lather of warm gruel or decoction of starch are also preferable to alkaline baths. Few remedies exceed in value the employment of lemon-juice locally in obstinate cases of palmar psoriasis. The part should be rubbed with a piece of freshly-cut lemon several

times a day. It is often difficult to prove the exact nature of chronic palmar psoriasis. Many cases are doubtless of syphilitic nature, but many are quite uninfluenced by anti-syphilitic treatment. Lemon-juice may be tried in all chronic and unyielding cases. (Dr. Dyce Duckworth, p. 300.)

SYPHILIS.—*Subcutaneous Injection of Mercury.*—The best solution for subcutaneous injection of mercury is one of three grains of the bichloride of mercury to the ounce of water, to which a little glycerine has been added. This is less irritating to the tissues than any other preparation. The following are the advantages of administering mercury in syphilis by subcutaneous injection rather than by the mouth:—1. The certainty and rapidity with which the symptoms disappear. 2. The small quantity of mercury necessary. 3. Exactness in the measurement of the dose. 4. Impossibility of disappointment through patients neglecting to take their medicine, &c. 5. Absence of gastric and intestinal irritations. 6. Economy in hospital use. 7. The avoidance of the publicity involved in using baths. 8. The means it affords of rapidly affecting the system in certain grave complications. (Mr. C. J. Cullingworth, p. 319.)

There are two principal causes of irritation attending the subcutaneous injection of the sublimate; (1) the known acidity of the solution, and (2) the property it possesses of coagulating the albumen of the tissues. To obviate these inconveniences Dr. Staub proposes to adopt a modification of Mialhe's "normal solution of mercury," which is said to be a solution of the sublimate in that form of combination into which all mercurials enter before they act on the system. It consists in adding to a simple solution of the bichloride in water small quantities of the chlorides of ammonium and sodium and the white of an egg. This solution, Dr. Staub says, is free from acidity, and will not coagulate albumen. It will, therefore, pass directly into the circulation without the formation of a coagulum at the seat of injection; and the delay and irritation due to the process of re-dissolving will be avoided. Forty-four cases of constitutional syphilis are appended, in forty-two of which a cure was effected very rapidly. The average number of injections given to each patient was thirty-four, and the total quantity injected a little under two grains and a half. Mr. Cullingworth having obtained a supply of the solution prepared according to Staub's formula, proceeded to test its effects. The usual dose employed was that recommended by Staub—namely, five milligrammes (one-fourteenth of a grain) of sublimate in a

gramme (fifteen drops) of fluid. The injections were generally given every morning, either in the upper arm or in the gluteal region. Fifteen of the cases were suffering from constitutional syphilis, and all these had some form of syphilitic eruption. In addition to the cutaneous syphilide, two of them had iritis, and one had gummous tumours, as large as an almond, situated in the substance of the back part of the tongue, which had been in a stationary condition for some months. In every instance the rash disappeared very quickly, the average time being eleven days. The cases of iritis were cured, one after seven and the other after eighteen injections, without any symptom of mercurialisation. The tumours of the tongue had entirely disappeared in ten days. The average quantity of sublimate required to produce slight tenderness of the gums was about half a grain; and in one instance slight salivation was produced after injecting two-sevenths of a grain. The pain caused directly by the operation was in every instance inconsiderable. The punctures were *always* followed by a certain amount of induration, which had not become absorbed when the patients left the hospital. (Mr. C. J. Cullingworth, p. 316.)

Dr. Levin, of the Charité Hospital, Berlin, has published some observations upon this subject, based on 700 cases. He used a simple solution of the bichloride in water (about four grains to the ounce), adding a little glycerine and morphia in the case of patients with a sensitive skin. Fifteen drops (one-eighth of a grain) were usually injected daily, sometimes in the side of the chest, and sometimes below the shoulder-blade, or on the outer side of the arm. It was exceedingly seldom (only in 2 or 3 per cent. of the cases) that he had to contend with local accidents of any importance—as, for instance, abscesses or sloughing of the overlying skin, and even then he attributes them to the clumsiness of the operator. Fifteen or sixteen injections, involving a total quantity of about two grains of sublimate, were generally sufficient to cause the symptoms to disappear; and he affirms that the relapses were considerably fewer than after ordinary mercurial treatment. His book concludes with a short statement of the advantages of the method:—1. Rapidity of cure (proportional to the quantity injected daily). 2. Comparative infrequency and lessened gravity of a relapse. 3. Precision as to dose. 4. Facility of administration. 5. Freedom from irritation of the alimentary canal. (Dr. G. Levin, p. 314.)

TERTIARY SYPHILIS.—It is generally admitted that parents suffering from tertiary syphilis beget strumous offspring—

which is but saying that the syphilitic poison is not sufficiently strong to reproduce itself, but has vitiated the blood sufficiently to determine the production of a strumous stock; and thus struma (rightly, as it seems to me) comes to be looked upon as a sort of bastard syphilis, or, better, as the quarternary form of the disease. The formula commonly adopted is that mercury is the remedy for secondary syphilis, particularly for the earlier manifestations, mercury and the iodides for late secondaries, and the iodides alone for tertiary syphilis, mercury being positively baneful. I venture distinctly to dispute the accuracy of this formula, on the ground of practical experience, and to affirm that, except perhaps in cases of extreme bone-mischief, mercury may be so administered as not in any way to impoverish the blood or injure the constitution, but with the sole effect of acting as an antidote to the syphilitic virus. The mode of administering the mercury is important. The internal exhibition of the drug is frequently ill borne, but either the mecurial vapour-bath or wearing a mecurial belt answers very well. Of the two, the belt is the most uniformly successful in these cases, as exhausting sweating sometimes follows the use of the bath. Neither inunction nor subcutaneous injection are admissible; indeed, I regard the latter plan from pretty extensive experience as eminently objectionable, being liable, in spite of the favourable testimony of Lewin and others, to produce alarming constitutional symptoms and troublesome local sloughing. I have also found that cases of secondary syphilis, treated with the subcutaneous injection, are (probably from the small quantity of mercury employed and the very rapid *apparent* cure) more prone to relapses than cases treated with mercury on the other plans. (Mr. S. M. Bradley, p. 319.)

VENEREAL WARTS AND CONDYLOMATA.—Mr. Berry relates a case in which he unsuccessfully tried the use of various escharotics, including fuming nitric acid. Almost despairing in being able to rid the patients of these pests, I resolved to try the acid nitrate of mercury (*liquor acidus hydrargyri nitratis*), though with little faith in its efficacy, after having failed with nitric acid. In both cases (females) the warts grew on the perinæum, around the arms, and on the skin and mucous membrane of the labia majora. I oiled the parts around and applied the acid nitrate of mercury freely, by means of a firm pledget of lint, intending to do so again daily if required. Next morning, however, to my surprise, the warts had become much shrunken and appeared to be covered with a yellowish white slough; the patients com-

plained of feeling very sore, and had been pained since the application. Poultices of linseed meal were now applied, and when the parts were cleaned the warts had almost completely disappeared (a second application removing them), and the skin where they had been was quite healthy. In some cases where the warts are one large granulating mass, giving forth an offensive discharge, removal with the *écraseur* will be required; but in those cases where they cover a large surface the application of the acid nitrate of mercury will be found to be the best remedy. What part the mercury plays in its caustic or escharotic properties I am unable to say, but certain it is that the remedy is superior to strong nitric acid. (Mr. W. Berry, p. 322.)

AFFECTIONS OF THE SKIN, &c.

CHRONIC ECZEMA.—*Carbolic Acid Lotion.*—In cases of chronic eczema, attended with much irritation and itching, the part affected may with advantage be first cleansed with carbolic acid soap and tepid water, in which is dissolved a little carbonate of soda, and then the following lotion applied by means of a sponge. Carbolic acid, one drachm and a half; glycerine, half an ounce; water, six ounces. In a case which had lasted for many years and had been unsuccessfully treated with arsenic and zinc ointment, and with many other remedies, the affection had entirely disappeared in two months under the plan of treatment recommended. In another case it was found that the lint stuck so tightly to the eczematous patches, owing to the amount of exudation, that the act of pulling it off irritated the skin. In this case an ointment of ungt. zinci, with carbolic acid and chloroform, aa min. xx. to ʒj. was applied, and the lotion, after a day or two, used as before. Under this treatment the disease gradually progressed until recovery was complete. (Dr. J. G. Thornley, p. 292.)

ERYSIPELAS.—*Subcutaneous Injection of Carbolic Acid.*—It appears not improbable that erysipelas is the result of the entrance of minute organisms into the subcutaneous connective tissue and of their multiplication. Acting upon this idea, the experiment has been tried of injecting subcutaneously a one per cent. solution of carbolic acid into places around the disease. It was found that the erysipelas did not spread in the direction of the part where the injection was made, and the fever and frequency of the pulse were at the same time reduced. (Dr. Aufrecht, p. 293.)

Camphor.—M. Revillout states that he has several times had occasion to employ with good effect in erysipelas an application used by M. Delpech at the Necker. It consists in painting the affected surface with a solution of camphor in ether (equal weights); and when this is employed in erysipelas of the face, and the affection has not yet reached the hairy scalp, its progress is usually arrested. It is also very useful in erythema, caused by local irritation. (*Gazette des Hôpitaux*, June 20.)

EXOPHTHALMIC GOITRE.—*Belladonna*.—Two cases of exophthalmic goitre are related, in both of which the distressing symptoms of the disease were fully developed, in which, after complete failure of other treatment, belladonna gave relief. The most probable theory of the disease is, that it is a paralysis of the cervical sympathetic, and the trial of belladonna was suggested by the statements of Dr. Harley and Dr. Meryon that belladonna is a stimulant to the sympathetic. The belladonna was given in doses of five drops of the tincture every hour, afterwards reduced to fifteen minims four times a day, and afterwards iron was added. The relief of the palpitation, of the quick, throbbing pulse, and of the profuse perspiring was, so to speak, immediate. The patient was restored to comfort and ease in a fortnight. Amelioration in other respects was gradual, and is still progressing. The diplopia was removed in six weeks. It was quite two months before decided improvement in the exophthalmos could be reported. (Dr. R. T. Smith, p. 297.)

FRECKLES.—For the benefit of young persons afflicted with freckles, it may be stated that powdered nitre moistened with water, applied to the face night and morning, will soon remove all traces of them. (*Druggist*, Feb. 1874.)

FAVUS.—Favus is a tedious affection, requiring patience on the part of the person suffering, and great painstaking with the medical attendant; but the following treatment will be successful:—In the first place, examine some of the crusts under the microscope to ascertain if any mycelial threads or fungous elements of the *Achorion Schönleinii* can be detected. If so, then adopt the following plan:—Take a very small pair of scissors, which will detach a crust or scab and leave a raw exuding surface; cut all the hair close to the skin—there is no necessity to epilate, as advised by some. Having obtained one raw surface, make a determination to cure that spot; wash it clean with a lotion of carbonate of potash (one drachm to three ounces of rose water); then dry with a piece of lint, and paint it afterwards with the following solution—viz., nitrate of silver, one scruple; spirit of nitric

ether, half an ounce. Now this is the plan which must be adopted:—Each day a fresh spot attacked and cured; and if a spot that has been treated on a former day exudes any moisture, paint again with the nitrate-of-silver solution. The child should wear a skull-cap of linen rag wet with the alkaline solution, covered with oil-silk during the day. Great cleanliness is necessary. Internally, good food, with liquor arsenicalis, and a purgative, such as calomel and jalapine, every other morning. Whilst administering the purgative, watch the secretions to see that they are healthy. Without it there is no utility in trying local treatment. (Dr. John Swift Walker, *Lancet*, Sept. 19, p. 436.)

Poultice twice a day with linseed-meal, having previously mixed it with the froth of porter (this will effectually remove the scabs in a few days); wash twice a day with an alkaline lotion; dry perfectly; then apply iodide-of-sulphur ointment twice daily, regulating the strength according to circumstances, and keeping the head covered with oil-silk. Internal treatment, if scrofulous, cod-liver oil; if not, give (Donovan's solution) liquor arsenicalis and hydriodate of mercury. Epilation I do not think is of any real benefit, as the disease does not originate from the hair-bulbs. I have seen it in two instances on the trunk. Cutting the hair very close is advisable and necessary. Give a seidlitz powder twice every week. (Mr. J. N. Bredin, *Lancet*, Sept. 19, p. 436.)

ONYCHIA MALIGNA.—*Nitrate of Lead*.—The application of dry nitrate of lead to the malignant sore, night and morning, is productive of the happiest results in this painful disease. This treatment, originally recommended by Mr. MacCormac, is favourably illustrated by some cases treated at Charing Cross Hospital. (Mr. F. Clarke, p. 306.)

PSORIASIS INVETERATA.—*Meat Diet*.—In a case of inveterate non-syphilitic psoriasis it is worth while trying the effect of a completely meat diet, as recommended by Dr. Passavant, of Frankfort. He had himself suffered for twenty-five years, from psoriasis, and had vainly tried every possible remedy, but was completely cured at last by this plan. Dr. Parkes relates a similar case. (Dr. E. A. Parkes, p. 155.)

TINEA TONSURANS.—The following plan will be found useful. A margin of healthy hair is to be cut quite short or shaved round the patch. A brisk rubbing with the ol. picis rect. or some similar hydrocarbon is the next stage, and the reddened and saturated patch is to be thickly dusted with a powder composed of tannin, iodine, and gum arabic. This is to be moistened with a few drops of the oil, and gently but firmly

pressed into the skin with the end of a small cork. Repeat the process till the whole patch is covered with a layer of paste about an eighth of an inch thick, and then allow it to dry. The firm hard scab thus formed may be left undisturbed for three or four days, when it should be moistened, scraped off, and reapplied. No home dressing is required in the intervals. This mode of treatment is not easily applied to patches of more than about an inch in diameter, as the artificial scab is apt to crack and fall off in pieces. The advantages of the method are the long interval between the applications contrasted with what can be had with any evaporable dressing in common use; the absence of irritation; the prevention of auto-inoculation by combing, &c., owing to the solid roof formed over the fungus; and the avoidance of bandages, pads, impervious night-caps, or other easily disarranged contrivances. (Mr. E. A. Browne, p. 305.)

VARICOSE ULCER OF THE LEG. — Mr. Hamilton, of Loughborough, Ireland, relates a case of obstinate varicose ulcer of the leg which showed no signs of healing, after prolonged rest and careful strapping, along with other local treatment. With the aid of a lancet he removed about a drachm of blood locally from the immediate neighbourhood of the ulcer, and repeated this on a second visit with the effect of the ulcer completely healing, simply by removing the local congestion. (Mr. C. W. Hamilton, p. 299.)

AFFECTIONS OF THE EYE.

CATARACT. — *Opening the Capsule before Making the Corneal Section.* — There are great disadvantages in the present way of operating, especially that directly the corneal section is made the aqueous rushes out, the cornea collapses, and the pupil contracts, however well it may have been dilated previously, and the cystotome has then to be introduced and the capsule lacerated. It is much better to lacerate the capsule before making the corneal section. The method of operating is as follows:—If the patient be not under the influence of an anæsthetic it will be possible, by fixing the eye in the manner employed in ordinary discission operations, to dispense with the use of the speculum and fixing forceps during the first step. It is an advantage to do without the fixing forceps if possible, as there is then less likelihood of an escape of aqueous humour on withdrawing the needle. There is nevertheless an advantage attending the use of the fixing forceps—viz., that the needle can be used

with more freedom and precision. The pupil should have been previously fully dilated by atropine. The needle used should be slightly curved near the point, and should be the finest possible in the shank. It is entered very obliquely at the lower and outer quadrant about 1" from the corneal margin. Its point is then pressed against the centre of the capsule, and a puncture made with a view to ascertain the consistency and nature of the cortex. This done, the operator decides whether to remove the cataract in its capsule or not. If he decides to lacerate the capsule, the point of the needle is carried round the upper and inner margin of the lens with a semicircular sweep, and then across the lower and inner margin with a second sweep, the two semicircles being joined above and below by separate movements of the needle if necessary. It is then withdrawn slowly and carefully; and if its passage through the corneal layers has been sufficiently oblique, there will be no loss of aqueous humour, and the pupil will remain dilated as before. If, however, aqueous has unavoidably been lost, the operation had better be completed on the following day, or even a week later. (Mr. W. S. Watson, p. 285.)

CORNEA AND IRIS, &c.—*Suppurative Affections of*.—In suppuration of the cornea and iris which do not yield to atropine and opiates, incise the cornea directly through the median part into the anterior chamber, just as you would do in a case of whitlow or thecal abscess. The patient having been put under the influence of an anæsthetic, an incision, in length about one-third of the diameter of the cornea, and penetrating the anterior chamber, is made by Graefe's knife. The incision should be made *not valvular*, but *vertical to the curve of the cornea*, traversing "in its longest diameter (Bader)," any abscess or specially diseased portion of cornea, and tending towards the pupillary portion of the cornea, where the risk of prolapse of the iris will be least. When the site or direction of the incision is not determined by a specially damaged spot, it is better to carry it across the middle of the cornea, slightly *above* the centre of the pupil. This suggestion to avoid the centre of the pupil is made, because I have not yet been able to ascertain whether an incision such as I have described can be made across the centre of the healthy cornea without impairment of vision. (Mr. T. P. Teale, Ophthalmic Hospital Reports, Oct., p. 62.)

INTOLERANCE OF LIGHT OCCURRING IN SYPHILITIC CORNEO-IRITIS.—The croton chloral hydrate is a remedy of great value in relieving photophobia, but from the experience gained in treating a number of cases with it, it appears that only

young people and only those suffering from genuine syphilitic corneo-iritis are benefited by it. The best dose is between five and ten grains three times a day, although in one case twenty grains was given four times a day, without producing much good or any bad symptoms. (Mr. Bader, p. 288.)

LACHRYMAL FISTULÆ.—*Injection of Iodine.*—In a case of fistulæ of the lachrymal sac, resulting from an abscess, pure tincture of iodine was injected by means of one of Anel's syringes. Inflammation of the internal membrane of the sac, extending to the conjunctiva, was the result. Fomentations with a solution of borate of soda were applied, and complete cure was effected in the space of four or five days. (Dr. del Toro, p. 288.)

PANNUS.—*Turpentine.*—Dr. Chisholm reports the following case:—A man, thirty years of age, was the subject of a dense vascular pannus in the right eye, which had been subjected to all the recognised modes of treatment, not excepting peritomy, without success. Dr. Chisholm prescribed the following drops, to be used once daily:—Oil of turpentine, one part; olive oil, two parts. After the first instillations, decided improvement was manifested; day by day the cornea became paler, by reason of atrophy of the pannus, until after some weeks it became possible to distinguish the colour of the iris and the contour of the pupil. By perseverance in this plan of treatment, the patient so far recovered that it was scarcely possible to discover which eye had been affected. (Richmond and Louisville Medical Journal.)

TRICHIASIS. — *The operation of Repositio Ciliorum.*—The principle of the operation consists essentially in causing the offending eyelashes to be mechanically turned away from the eye, and made to grow more or less in the proper direction by making them pass under a narrow bridge of skin. The following is the method of performing the operation:—A very fine curved needle has the two extremities of a very fine waxed silk ligature (or hair, as Celsus directs) passed through its eye. The needle, being firmly grasped by suitable forceps, is then passed through a narrow fold of skin at the very margin of the lid, close to one of the inverted eyelashes. The point of introduction should be external to the point of emergence of the eyelash, but as close to it as possible; and the needle should be brought out after passing about $\frac{3}{4}$ " or 1" under the skin. The needle and ligature should be drawn through until a small loop alone remains, when, by means of a fine pair of forceps, the eyelash is passed through the loop. Traction is then made on the ligature, and the loop with entangled eyelash, is drawn

through the tunnel of the skin. The other misdirected eyelashes are similarly treated. The chief difficulty in the performance of the operation consists in getting the eyelash entangled in the loop of the ligature, as the bleeding which occurs from the points of puncture causes the eyelashes to be matted together, or to adhere to the ligature, and the misdirected eyelash is almost always enveloped by a small clot. When this source of annoyance is troublesome, it is advisable to wait a minute or two after the passage of the needle before proceeding to the ensnaring of the eyelash. In some nervous patients, I have found it advisable to introduce the lid-forceps or clamp, so as to stretch the lid and enable it to be more readily fixed, and thus ensure accuracy in the points of introduction and emergence of the needle. One thing that must be carefully attended to is to make the point of introduction of the needle as close to the offending eyelash as possible, as, if it be made at a little distance, the resiliency of the hair causes it gradually to emerge from the tunnel and resume its former position. This operation is, unfortunately, only applicable to those cases of partial trichiasis in which the eyelashes inverted are few in number and of considerable size. (Dr. D. A. Robertson, p. 282.)

MIDWIFERY, &c.

FIBROUS TUMOUR OF THE ANTERIOR LIP OF THE UTERUS.—*Removal by the Galvanic Knife.*—Dr. Atthill relates a case of removal by the galvanic knife of an ovoid tumour the size of a hen's egg projecting from the vagina, but really growing from and being as it were part of the anterior lip of the uterus. The apparatus employed was Grenet's. The galvanic knife consisted of a loop of platinum wire about half an inch in length, connected by means of the ordinary wire conductors with the battery. The cervix measured at the point selected for amputation $3\frac{1}{2}$ inches in circumference. The great thickness of the tissue to be divided, and its extreme denseness, rendered the operation very tedious; it occupied in all thirty-five minutes. The slow progress made in dividing the tissues was also in no small degree due to the vascularity of the part, the flow of blood on each stroke of the knife being so considerable as to cool the platinum wire to such a degree, that ten or fifteen seconds frequently elapsed before the knife was hot enough to be again used. The cauterization was, however, sufficient to prevent any serious hemorrhage occurring; still two arteries had to be ligatured. Excepting what occurred from these two vessels, the cauterization effected by the knife was sufficient to check the hemor-

rhage, nor was there any subsequent loss of blood. The plan, however, was not entirely satisfactory, on account of the great length of time which it occupied (35 min.), which more than counterbalanced the advantage obtained in lessening the risk of hemorrhage. The smell, too, produced by the slow cauterization was most disagreeable. (Dr. L. Atthill, p. 360.)

HYSTERIA.—*Phosphorus*.—It seems probable that phosphorus will prove a remedy of considerable value in cases of hysteria, which are indeed really cases of nerve depression. The proper dose is about one-twelfth of a grain of phosphorus, dissolved in cod-liver oil, or in alcohol and glycerine. This medicinal treatment should be supplemented by such general treatment as the case may require. (Mr. J. A. Thompson, p. 57.)

INDUCTION OF PREMATURE LABOUR.—The old plan of puncturing the membranes and then giving ergot is most unsatisfactory, inasmuch as it is reversing the process of nature. We ought to dilate the os first, and this can only be effected by tents of sea tangle or compressed sponge, and Barnes' india-rubber bags. My experience leads me to prefer the tents, and the following is the manner of inserting them. The patient being in the ordinary obstetric position, the fore and middle fingers of the left hand, well oiled, are to be passed up to the os uteri externum, which is generally sufficiently open to admit the point of one, and sometimes even of both fingers. A small-sized sponge-tent, about the size, for instance of an ordinary radish, is to be mounted on a proper stilette, made for the purpose, and passed along the groove between the fore and middle fingers of the left hand, until its point is inserted into the os uteri. The tent is then to be pushed on until a very little of it, not more, perhaps, than a quarter of an inch, projects into the vagina beyond the os uteri. If the os be so high that it cannot well be reached by the finger, the tent may be inserted through a full-sized tube speculum. If also it be very little open, a tangle-tent may be found more convenient to begin with. Care should be taken to push the tent up sufficiently far, as it should be remembered that, two months or so before delivery, there is often a considerable interval between the os uteri externum and internum; otherwise the tent may not pass through the os internum. After the tent is introduced, a piece of soft silk handkerchief may be passed up as a plug to keep it *in situ*. On the day following, this tent should be removed and a large one inserted, and on the next day a tent of the largest size. Generally two, or at most three, tents

are required to complete the full dilatation. At this time, probably labour-pains will set in regularly, and the membranes will rupture. Should this not be the case, the membranes may be punctured, and the most convenient instrument for this purpose I have found to be a gum elastic male catheter, of which the end has been cut off, so that the stilette can be protruded at pleasure. Should the pains still be weak or ineffectual, ergot may be given. Before it was the custom to impregnate sponge-tents with carbolic acid, they used to become very offensive if left in the vagina more than ten or twelve hours, and there was some risk of septicæmia in consequence. I, therefore, never was accustomed to leave a tent *in situ* more than twelve hours. In this time, however, the tent will not dilate to its full extent. I now always leave each tent twenty-four hours *in situ*, and I do not find that there is any risk of septicæmia when carbolised tents are used. On the whole, I prefer carbolised sponge-tents very much to the elastic bags, because they effect the process of dilatation more steadily, continuously, and gradually, and therefore imitate more exactly the natural process of dilatation, which frequently occupies several days from its first commencement. As a general rule, it is not desirable to dilate the os too rapidly, especially when the time for that operation falls considerably short of the full term, for then there is a proportionally greater thickness of the os uteri to overcome. (Dr. J. G. Swayne, p. 325.)

INTRA-UTERINE STEM PESSARY.—Dr. Clement Godson, of the Samaritan Free Hospital, uses an intra-uterine stem made entirely of aluminium, and so light that, when placed on the palm of the hand at some distance from the mouth, it can with very little effort be blown off it. This extreme lightness causes the instrument to have but little tendency to get displaced. But should this arise, a spring is pushed up the canal of the stem, and expands through two lateral apertures near the top of the stem, so fixing it in position. The use of this, however, is rarely required, and is to be avoided if possible. (Dr. C. Godson, p. 363.)

OVARIAN DROPSY.—*Diagnosis.*—In the diagnosis between ovarian dropsy and the other conditions which resemble it, all the senses must be used in obscure cases. By the uterine sound much can be learned, as well as by vaginal and rectal digital examination; but, of all these, palpation and percussion afford the most valuable information. By passing the hand over the abdomen, the presence of a tumour is ascertained, also whether it is solid or fluid, or partly the one and partly the other; whether it is movable or immovable,

smooth and regular in its outline or the reverse; but, by percussion, it must mainly be decided whether the swelling is the result of ascites or of ovarian disease, or caused by tympanites, or by a phantom tumour. In ascites, the stomach and intestines, containing air, float on the surface of the fluid, and, therefore, the highest points of the tumour, the patient lying on her back, give out a clear sound on percussion. If, however, the fluid be contained in a cyst, the stomach and intestines are pushed aside, as the tumour rises in the abdomen, and lie in the epigastric and two lumbar regions. Hence, the highest points of an ovarian tumour emit a dull sound when percussed, and the epigastric and lumbar ones give a clear sound. By applying these general rules in any ordinary case, a few seconds will enable a surgeon to clear up all doubt. The abdomen in ascites may be so distended that the intestines cannot float on the surface, or the intestines may be tied down by adhesions, the result of peritonitis, probably of a tubercular, and possibly of a malignant character, and itself the cause of the ascites. Such conditions might without care lead to errors in diagnosis. There is a condition, however, to which Mr. Wells does not allude, but which is mentioned by Peaslee, which is a most invaluable means of diagnosis. This is a prolapse of the posterior wall of the vagina, from the pressure of the fluid in Douglas' space. It is the upper portion of the posterior wall of the vagina that is prolapsed, the part covered by the recto-vaginal pouch of the peritoneum. It is easily distinguished from the tumour formed by prolapse of the lower part of the vagina known as a vaginal rectocele, by passing the finger into the rectum and thence into the pouch from which the tumour receives its name. We have scarcely ever seen a woman whose abdomen was much distended by ascites in whom this prolapse had not occurred. It is also found where there is an enlarged ovary or other tumour surrounded by ascitic fluid, but never in cases of simple ovarian or other tumour. (Mr. Spencer Wells, Dr. Peaslee, p. 364.)

POST-PARTUM HEMORRHAGE.—If the placenta is removed, or, to put it in another way, if the uterus, with a little assistance, is caused to expel the placenta immediately after the birth of the child, we considerably diminish the risk of post-partum hemorrhage, and should it set in, we have the uterus more immediately under our control. *Immediately* on the birth of the child the uterus is to be firmly grasped and pressed downwards in the axis of the pelvis, and the right index finger being hooked on to the edge of the placenta, which is invariably in the vagina, with a gentlerotatory movement it can be extracted.

It is not right to teach that we should wait for a griping pain or two to effect the expulsion of the afterbirth. These so-called griping pains should only cause the firmer contraction of the womb *after* the expulsion of the placenta. (Dr. Bleunerhassett Atthill, p. 350.)

Arrest of Post-partum Hemorrhage by Vomiting.—Vomiting is a powerful agent in stopping post-partum hemorrhage. This may arise from the vascular sympathy which exists between the uterus and the stomach. Firm contraction of the uterus generally occurs after vomiting, and so much so is this the case that it is well to use common mustard as an emetic. (Dr. Ringland, Dublin Journal of Med. Science, May.)

Perchloride of Iron.—We ought not to have recourse to this remedy unless it is urgently required, as other modes of treatment are safer, and, generally speaking, more effectual; but cases may occur in which ordinary modes of treatment prove unavailing, and in these we may grasp at the perchloride as a dernier ressort. (Dr. E. Kennedy, p. 348.)

[We are surprised to find no allusion in these cases to the plug. Why be afraid of using the plug in uterine hemorrhage? We have had the most fearful cases of hemorrhage, but never remember to have lost one. Our practice is very simple. Clear out the placenta and all clots, run the fingers up the outside of the uterus so as to grasp the fundus, pushing all folds of intestine upwards for fear of injuring them. Pull the fundus downwards whether it will contract or not, now plug the vagina *right well* so that no blood can possibly escape. What is the result? You have the fundus uteri well down, you have stopped up the opening below,—you cannot have a coagulum much bigger than two fists. This very coagulum acts as a plug in the womb; the *bleeding vessels cannot bleed through a coagulum*. You have thus effectually plugged the bleeding vessels and can wait; but you must keep your hand, or rather fingers, well grasping the fundus, else the womb may dilate again. In a little time, one, two, or more hours, remove the plug from the vagina, press out the coagulum from the womb and see if hemorrhage begin again, always giving, as soon as possible, a large dose of ergot. We think the ergot is given in far too small doses; we generally give a half-ounce bottleful of the powder well infused, or two drachms of the liquid extract. We consider all the prejudices against the plug perfectly unworthy of notice. Of course if the hemorrhage return, we plug a second time. There is simply no necessity whatever to use the injection of iron, and for our own part we should always disapprove of it.—EDS.]

THE LONG MIDWIFERY FORCEPS AS A COMPRESSOR.—By making the long forceps rather heavier, and attaching a rack and pinion to the handle, connected by a screw which regulates the amount of pressure exercised on the child's head, we have an instrument greatly increased in power, and capable of useful employment in cases where the ordinary long forceps would be of no value. It is possible to compress a child's head with the forceps sufficiently to enable it to be extracted through a pelvis rather smaller than its natural diameter. No argument is necessary to prove the extraordinary compressibility of the foetal head. Every tyro in midwifery is cognisant of the extraordinary manner in which the child's head may be moulded or lengthened out by the pressure to which it is subjected during protracted labour, without any permanent injury, and the intention of using a compressing forceps such as mine is to imitate and supplement this natural moulding process in cases where the *vis à tergo* does not suffice to overcome some obstacle to delivery. Living children have been extracted through pelves measuring three inches in their widest diameter; therefore it does not follow that because a woman's pelvis is small, or somewhat deformed, she must be delivered by craniotomy or cephalotripsy. On the contrary, I have myself recorded cases which prove that even under these circumstances the forceps may sometimes be used with success; and in no less than twenty-nine of my forceps operations there was some degree of disproportion. For extracting the head when low down in the pelvis the short straight forceps answers better than one with a double curve. In using the long forceps it will be found easier to introduce the left-hand blade first, whereas with the short instrument the upper blade is first applied. Before passing it the instrument may be locked so that the operator can see that the blade he selects first will look forward when *in situ*, or in other words that its curve will correspond with that of the pelvis. When the head is high up above the brim the hand should be introduced till the ear is felt, not that it is always necessary to apply the long forceps in this situation, but it is always necessary to guard the uterus from the instrument, which should be cautiously passed between the operator's hand and the child's head in the transverse or oblique diameter of the brim of the pelvis. The introduction of the second blade and locking require no further observation. When properly adjusted, though locked, the handles of this instrument should not come closely together; they may therefore be very slowly and gently brought nearer each other by the screw at their extremity, but no force is to be used, nor any violent effort

made to approximate them under any circumstances. (Dr. T. M. Madden, p. 332.)

VARICOCELE OF THE LABIUM.—A varicocele of the labium gives rise to much discomfort from the excoriations and discharge produced, but its removal is attended with great danger from hemorrhage. By the use of the elastic ligature, however, this danger may be avoided. A case is related in which an operation had to be abandoned and the patient watched day and night by dressers, but in which the use of the ligature at once led to a successful result. (Mr. L. Tait, *Lancet*, June 27, p. 905.)

VOMITING OF PREGNANCY.—*Hydrochloral by the Rectum.*—Dr. Simmons, of Yokohama, Japan, states that in a case of vomiting of pregnancy in which all the remedies used had failed, he suggested the administration by the rectum, morning and evening, of thirty grains of hydrochloral on mucilage, and this to be increased if there was no improvement, or if the specific effect of the medicine was not too decided. An amelioration of the symptoms was obtained by the first injection, and a still more satisfactory one followed the administration of the second. The second day's use of the remedy arrested the vomiting, except at long intervals, and on the third day both nausea and vomiting ceased entirely. There was no return of the symptom. (Dr. Simmons, p. 328.)

By Dr. THOMAS B. PEACOCK, Senior Physician to St. Thomas's Hospital.

[Typhoid fever is a disease which by preference attack people who are comparatively young, the average age of 65 cases which have occurred at St. Thomas's Hospital during the last two years being only 21.8.]

One of the most characteristic features of typhoid is the eruption which generally appears upon the skin. The spots are of a pale rose colour, and fade on pressure. In size they vary from half a millet seed to less than that of a split pea. They are rounded though not distinctly margined, are flattened on the surface, and give no feeling of hardness or resistance to the finger passed over them. They do not, however, occur in all cases, vary considerably in number, appear at irregular periods, and are of very uncertain duration.

In one case the typhoid spots were combined with an eruption of a distinctly petechial character. In two or three cases the characteristic spots were combined with a patchy roseolous eruption. In one case their appearance was preceded by a lichenous eruption covering the trunk, and in another instance a similar lichenous eruption appeared in a case in which there were no typhoid spots. In one case their appearance corresponded with an outbreak of herpes about the mouth. In many cases they were succeeded towards the end of the attack by an eruption of miliary vesicles, or sudamina. These are generally found about the lower part of the abdomen, and at the folds of the axillæ and the root of the neck and upper part of the thorax. At first the spots are small and round, and the contained fluid is perfectly clear. Subsequently they increase in size, are flattened, and contain an opaque fluid, and still later they break, and there is a slight desquamation of the skin in the parts where the sudamina have been. Usually the appearance of the miliary eruption corresponds with the out-

break of partial sweats in some parts of the body, though the situation they occupy is often dry and harsh. They do not possess any diagnostic importance, as they appear in other diseases as well as in the specific fevers, as in acute rheumatism and hectic; but they are more common in typhoid than in typhus, and are especially frequent in hot weather. Independently of the eruptions the state of the skin in typhoid is somewhat uncertain; generally it is dry and harsh during the height of the disease, but this is not always the case, and I have several times known a patient perspire somewhat profusely during the whole course of the attack. With the progress of the fever the temperature rises—in slighter cases to 102° or 103° , in the more severe to 104° or even 104.5° , and it is generally higher at night than in the morning. This temperature may be attained in the second or third week and may continue for a week or ten days, when it usually begins to fall, the declension being especially marked in the mornings. The temperature also often long continues above the normal standard, and readily rises again from any slight cause, even when convalescence is well advanced. The pulse is accelerated throughout the attack. In the earlier period it may rise to 110 or 112; in the more advanced to 120; and in severe cases it often becomes very rapid and feeble; the increased frequency of pulse generally being continuous during convalescence.

The condition of the tongue in typhoid is very variable. Most commonly in the early stage it is covered with a whitish brown fur and somewhat dry; at a later period it becomes very dry, and is covered with a thick brown fur, the tip and edges being often at the same time red and sore-looking. There are also sordes on the teeth and the lips are parched. In other cases, however, the tongue has only the whitish brown fur and is not quite dry during the whole illness; and in yet others the tongue is throughout morbidly red, glazed, and fissured. Usually, as convalescence advances, the fur gradually recedes from the tip towards the root of the tongue, leaving the external surface red and sore-looking; but sometimes the coating is rapidly shed and the whole surface assumes the same condition.

The abdominal symptoms are generally predominant. At the commencement of the attack there is usually more or less diarrhœa, the bowels being moved from three or four to six or eight times in the twenty-four hours, and the stools are of a pale yellow or greenish or brownish colour, and are almost entirely fluid. With the progress of the disease the diarrhœa generally subsides, the stools are passed less frequently and become more consistent, and at a still later period the relaxa-

tion generally ceases and is replaced by constipation. In some cases, however, the diarrhœa will be yet more severe, so that the bowels may be moved every few minutes. There may be blood in the stools, and the evacuations may be passed in bed, either from the patient being unconscious or from his being too weak to retain them. In yet other cases, on the contrary, there may be an entire absence of diarrhœa, evacuations only being procured, during the whole course of the fever, by the exhibition of aperients or of enemata. Of the cases reported upon, in several there was no diarrhœa, and in one of these there was obstinate sickness and vomiting, and in two others the same symptoms occurred, though the bowels were also relaxed. In four cases blood was discharged by stool. In one of these the hemorrhage occurred on the fourth day after admission and the sixth from seizure, but the patient had been ailing for a longer time, and death rapidly ensued. In another case the bleeding did not take place till the twenty-ninth day from admission and the forty-third of illness, and the patient survived three days. The other two cases terminated favourably, though in one of them the patient bled profusely from the nose and also vomited blood. The hemorrhage occurred two days after admission or on the thirteenth day of illness. In the other case the bleeding was in progress when the patient was admitted after a week's severe illness but he had been ailing before. In a fifth case which recovered the patient had passed blood in the stools before admission after being three weeks ill, but he had no bleeding afterwards. It is well known that epistaxis is a very common symptom at the commencement of typhoid, and I have known in several cases very large quantities of blood so lost. In two such instances, not included in this calculation, the patients would certainly have bled to death had the nostrils not been plugged.

There is very generally some degree of cerebral disturbance in typhoid, though often this does not amount to more than that the patient is so torpid as to be with difficulty aroused to answer questions, and generally there is some delirium at night, and he is usually very deaf. In some cases, however, very active delirium occurs, especially at the commencement of the attack. Indeed a state of active mania is much more common in typhoid than in typhus. In four of the present series of cases there was violent raving, though in all the patients recovered. In several of the cases, and especially in young persons, there was often marked impairment of mental power and great fractiousness long after convalescence was otherwise well established. In two cases, admitted during protracted convalescence, there were also cerebral symptoms.

There is generally in typhoid some congestion of the bron-

chial mucous membrane, indicated by a troublesome cough and by sonorous and sibilant rhonchi in the front of the chest and some subcrepitation posteriorly; and occasionally the signs are combined with indications of collapse of a portion of the lung tissue. These symptoms, however, generally subside as the convalescence advances and the patient gains strength. More severe pulmonary disease may however occur, and in one of the cases reported upon there was extensive pleuro-pneumonia, and in another general capillary bronchitis. Both patients recovered.

It is by no means uncommon for the urine at the height of the fever to become slightly albuminous and to be retained. The latter symptom is reported to have occurred in one of these cases.

It is one of the peculiarities of typhoid also that there is a peculiar tendency to relapse and for all the symptoms of the disease to recur, even after the patient has apparently become quite convalescent. In two of the cases reported upon relapses occurred; in one on the thirty-first day after admission and thirty-eighth day from seizure; in the other on the thirty-third day from admission and the fortieth from seizure. In both cases there had been the usual eruption on the skin, and in one of them there was a fresh outbreak of spots. Both patients ultimately recovered. One patient was also admitted into the hospital a week after the commencement of a relapse. He had been first attacked a month before and had become to a considerable extent convalescent. In this case also there was a reappearance of spots. The patient recovered. In several cases there were bedsores, or abscesses formed in different parts of the body during convalescence. One patient was admitted with embolism of the femoral vein, and another with contraction of the lower extremities, and a third with a tumid abdomen threatening marasmus, the result of attacks of typhoid.

In a case which I recently saw in consultation with Dr. Greenwood, of Dalston, a female, thirty-four years of age, had pyæmia after typhoid, followed by inflammation and rapid suppuration and collapse of one eye, and died in a few days. Recently also I saw a boy about ten years old in whom an attack of typhoid assumed a well-marked remittent type. He first suffered from diarrhœa, with delirium and other symptoms of fever. When I saw him, about the sixteenth day, there was still some diarrhœa; the abdomen was tumid; his intelligence was much impaired, and he was greatly prostrated; there were some spots on the abdomen, and others subsequently appeared. A day or two after this he began to have very marked exacerbations at about two o'clock in the

afternoon and the same hour in the morning, during which his temperature rose to 104° and 105° . These attacks were followed by states of excessive exhaustion in which his temperature fell below 98° , in some of which he was apparently on the point of death. He was, however, rallied by the very free exhibition of stimulants, and under the use of quinine ultimately recovered. The attacks were more marked on the alternate days, and, though they gradually subsided, he continued to have some increase of fever at the stated periods even when convalescence was well advanced.

It is probable that the appearance of the eruption in the skin corresponds with the progress of the local disease in the alimentary canal. Generally shortly after the spots cease to come out, which may be regarded as indicating the completion of the febrile process, there is an improvement in the condition of the patient. As, however, the injury which has been sustained by the bowel is only slowly recovered from, a state of subacute fever continues for a considerable time; the rapidity of the convalescence being influenced by the extent of the previous disease, by the degree of prostration caused by the fever, and by the existence or absence of other local complications. The disease is thus of very variable severity and duration. Sometimes it is only a slight affection which quickly subsides; in other cases it is very intense and protracted; and in yet other instances, though the fever may never be severe, it may yet be of very long duration. It has, indeed, been clearly shown by different observers that the constitutional disturbance cannot be regarded as indicating the amount of local disease, but that the fever is to be regarded, in the language of the older writers, as an *essential* one. The intestinal disease, though it very much aggravates the symptoms of typhoid, is, indeed, only a result of the fever, the relation between the two being precisely analogous to that what obtains between the fever and the eruption in variola.

The following may be regarded as the ordinary course of a case of typhoid of medium severity in which the patient recovers.

The patient, at the time he comes under observation in the hospital is usually able to walk to bed, and he has been more or less ill for from seven to fourteen days—usually about ten days. At that time the bowels are relaxed, the stools pale and liquid, and passed several times daily. The abdomen is somewhat tumid, and there is generally some pain and tenderness, especially at the lower part of the right side. The spots are then often present or come out soon after. The tongue is covered with a whity-brown fur and somewhat dry; or it may be brown on the dorsum, and red and sore-looking at the tip

and edges; more rarely it is clean, morbidly red, and fissured. The skin is usually somewhat dry, but it may be moist or profusely perspiring. It is warm, the temperature reaching often 100° or 102° , or to 103° or 104° , and being somewhat higher at night than in the morning. The pulse ranges 100 to 108 or 120. There is a slight flush on the cheeks, the lips are parched, the eyes heavy, and the patient is oppressed and restless, and slightly delirious at night.

During the next week or ten days the patient is much prostrated, and lies immovably on his back. The face is flushed, the expression of countenance very heavy and the eyes dull, the lips are parched, and the tongue is generally dry and brown, or brown on the dorsum and red at the tip and edges. The temperature continues much as before; the pulse is about 120, and weak; the bowels are relaxed, and the abdomen is more tumid. The spots still continue to come out a few each day. The skin is generally very dry and harsh, but sometimes continues moist. The patient is more torpid, and has low muttering delirium at night, and is so deaf that he requires to be spoken to in a loud voice; but when aroused can answer simple questions and protrude his tongue.

After about a week or ten days the spots cease to appear, and soon after there is an improvement in the general condition; the patient becomes less prostrated, he hears more readily, and answers questions more quickly and intelligently. The diarrhoea ceases, and is, perhaps, replaced by constipation. The temperature falls, especially in the morning, so that there is a very marked distinction between the temperatures of the morning and evening. The tongue becomes less dry and cleaner, the fur generally disappearing gradually from the point towards the root. Sometimes the tongue becomes suddenly clean, and is left morbidly red and sore-looking. The skin also loses its dryness and harshness, and miliary spots often appear in different parts of the trunk, and coincide with partial perspirations. On or about the twenty-first day the convalescence is generally well established, and the patient recovers; but the convalescence may be very prolonged by the extreme weakness of the patient, or from the occurrence of some local complication.

In some cases the whole train of symptoms is less marked, the tongue may never become dry and brown, and the temperature may not be high, the diarrhoea is less, and the convalescence may be established about the 14th day. In others the patient may not become convalescent before the 28th or 35th or 42nd day, or even later. The duration of the disease is also by no means to be measured by the apparent severity of the attack, some cases which have very urgent symptoms

being only of short duration; and others, in which the symptoms are slight, being on the contrary very prolonged.

During the whole course of the disease also, and however slight its apparent character, there is danger of the sudden accession of severe symptoms. Hemorrhage may occur and may destroy or imperil life; peritoneal symptoms may come on from extension of inflammation from the mucous membrane to the serous coat of the intestines or from decided perforation; or serious inflammation may occur in the brain or lungs.

If the patient dies, death may occur as early as the eighth or ninth day, but most usually it takes place in the second, third, fourth, or fifth week, and in some cases at a still later period. In the cases which prove fatal at the earlier period the aggregate and solitary glands at the lower part of the ileum will be found much enlarged, the mucous membrane over them inflamed, and yellow sloughs may occupy a larger or smaller portion of the surface. In the intermediate period there will be found active ulcers with abrupt edges in some places, especially near the ileo-cæcal valve, and partially separated sloughs in other parts, particularly higher up in the canal. At the more advanced periods there will be the remains of ulceration in the process of healing, the edges of the ulcers being depressed and the surface not deeply excavated; and at a still later period nearly all evidences of disease may have disappeared.—*St. Thomas's Hospital Reports*, 1873, p. 17.

2.—THE NATURE AND ORIGIN OF TYPHOID FEVER, CONSIDERED MORE ESPECIALLY WITH REGARD TO ITS PREVENTION AND TREATMENT.

By the EDITOR OF THE MEDICAL TIMES AND GAZETTE.

In considering the origin of typhoid as a specific disease, spreading only by its own proper poison, two difficulties meet us. First, we know not the exact period of incubation in some cases that appear to be very short, in others it apparently lasts weeks; and, secondly, we know not how long the specific germ, which is capable of causing the fever, can exist in a state capable of renewed growth outside the body. Here we take it for granted that the disease is occasionally propagated in the fashion above indicated; and that this is true, few will, we think, dispute. Nay, more, it can be fairly shown that this is so in a great number of instances—perhaps even the majority of instances, though not meantime in all. As already pointed out, those instances where typhoid fever is supposed to have arisen *de novo* are in many cases unsatisfactory as regards history. All loopholes have not been closed excluding the possi-

bility of a specific poison introduced in the ordinary way, and a good many of them were observed at a time when the idea of a specific agent invariably acting as the cause of typhoid fever was not generally entertained. In truth, the so-called pythogenic theory cannot in the majority of cases be for a moment entertained. We are all acquainted with places constantly saturated with evil odours of all kinds, including not unfrequently that of decomposing fæcal matter, and yet there is no fever. Horrible though it may seem, these matters make their way into drinking water, and yet no typhoid fever follows. To excite an outbreak of typhoid something more is necessary, and the question is—What is this something? Without doubt it has been shown by the researches of Drs. Budd, Ballard, and many others that the special exciting cause is typhoid excreta; and if so in a certain number of instances, there is a strong probability that it is so in all. In the typhoid stools undoubtedly are to be found the germs which propagate typhoid in a certain number of instances; and if putrid animal matter will not alone generate typhoid, it certainly constitutes a nidus, in which these germs thrive and apparently retain their vitality for a long period of time. The nature of these germs is unknown to us, but, inasmuch as they retain their power of self-propagation, they must consist of particles more or less minute of living matter; and it is certainly of importance not to confound the half or wholly dead masses of cheesy glands eliminated in the course of the fever, and giving the characteristic appearance to typhoid stools, with these active organisms. In point of fact, it seems to us that one great difficulty in the way of the universal acceptance of the germ theory as the cause of typhoid has been this difficulty of appreciating the early period at which these typhoid stools begin to contain the germs of the malady. Those who have seen much of typhoid fever know well the cases where, beyond a diarrhoea, the patient seems, comparatively speaking, fairly well; at all events, is able to walk about, and perhaps to carry on his usual avocations. In such a patient the symptoms may suddenly take a serious turn, showing that the malady has inappreciably reached a considerable height whilst altogether unsuspected. Such a patient may have been, unknown to himself and others, spreading the fever broadcast, for everything points to the fact that the original focus of the disease may contain only a small quantity of infective matter to give rise to the most serious consequences. Nothing could show this more clearly than the curious epidemic caused by the milk of a dairy company in Marylebone, the history of which was so well worked out by Dr. Murchison. We are only, too, beginning to appreciate the strong vitality of these germs

under favourable circumstances. Liebermeister, in a capital article on the subject of typhoid, in Ziemssen's newly-published "Handbook of Medicine," in which substantially the same view as that advocated by us is adopted, quotes from Von Gietl an exceedingly instructive illustration of this. In a village where for a long time no typhoid had prevailed, the disease was introduced by a patient who had acquired the disease in Ulm. The patient's discharges were thrown on the dust-heap, and when some weeks later this was in part removed, of five persons engaged in its removal four took unmistakable typhoid. The discharges of these persons were buried deep in the same dunghill, but when nine months later this was entirely removed, one of the two men so employed was seized with typhoid and died. These two sets of facts go far, in our opinion, to explain the difficulty heretofore experienced in connecting outbreaks of typhoid with direct introduction of typhoid germs. The only other argument advanced is, comparatively speaking, worthless; it is urged that such sequence can rarely be traced in towns and large cities. This is quite true; but when we consider the universal prevalence of the water-closet system, with drainage more or less imperfect, the ventilation of sewers through cisterns and into bed-rooms, together with the more or less continued prevalence of typhoid fever in some part or other of large towns, our wonder at this ceases—in fact, is turned the other way, for it seems much more wonderful that we should ever find out the exact cause of an outbreak of enteric fever than that we should sometimes miss it. Moreover, we are only beginning to appreciate certain agencies in propagating fever. Milk is now one of these, but we shall be surprised if it stops there.

[Typhoid fever is a specific form of disease, generated by a specific poison. In the management of it we must not only do our best for the individual, but it is also our duty to prevent, as far as possible, any spread of mischief among the community at large.]

We have in the meantime no specific remedy which we can rely on for cutting short enteric fever, and no substance we can be quite sure of for destroying typhoid germs within the system. As far as specific treatment is concerned, only two substances have of late been characterised even approximately as such—these are calomel and iodine. On the Continent, especially in Germany, where enteric fever is even more common than with us, the use of calomel is supported by authorities like Traube, Wunderlich, and Niemeyer, who hold that it not only does good generally, but even helps to cut short the fever. The dose given is from six to ten or fifteen

grains three or four times in the course of the first twenty-four hours after the patient comes under treatment, if this be fairly near the commencement of the disease. Iodine in iodide of potassium (our liquor iodi) has also been used, and has been asserted to be of great service. Certainly statistics on the larger scale seem to show that cases so treated do on the average much better than those left to take their own course; but here, as in many other respects, statistics are but poor guides, and there is notoriously one element they do not include, which in enteric fever is nearly all in all—that is, nursing and subsidiary management. Suffice it to say that the weight of authority is in favour of giving one or two doses of calomel if we see the patient in the early stage of the malady.

When we turn to the consideration of the practicability of destroying the poison within the system, one or two noteworthy facts suggest themselves. First, as already pointed out, the poison is almost certainly mainly propagated in the alimentary canal, and is therefore much more than ordinary animal poisons within the reach of medicaments. Experience has shown that certain substances, like green sulphate of copper, creasote, and carbolic acid, Condry's fluid, &c., have the power of destroying the germs of the disorder outside the body. Why should they not be used inwardly? To this the reply is, that there is no particular reason why they should not in moderate quantity: but experience is wanting as to their efficacy in arresting the production of fever germs, or in destroying those already formed. Another suggestion, however, occurs; it is well known that the pea-soupy stools of confirmed typhoid are invariably alkaline, and it is a question how far the germs which these contain can exist in presence of a strong acid. Moreover, experience declares itself on the side of giving acids in the disease, since they tend to remove the clammy condition of tongue which adds so greatly to the sensation of thirst, and to prevent severe diarrhœa. For these latter reasons alone we should be justified *nihil contradicente* to order dilute or aromatic sulphuric acid, or dilute hydrochloric or nitro-hydrochloric acid in ordinary doses from time to time, having a hope that the same remedies might tend to limit the spread of the disease by limiting the growth and number of the typhoid germs.

Practically, however, we are brought to this pass. We know of no assured means of cutting the malady short; we must, therefore, let it take its course, being careful to look out for those complications which are prone to be fatal if not sharply dealt with, but which are in reality fairly amenable to treatment. Foremost amongst these is excessive bodily heat—not hyperpyrexia in the sense the word has come to assume

(a temperature of 106° Fahr. or more), but what would be deemed by some to be ordinary pyrexia. All are agreed that hyperpyrexia is a thing to be dealt with sharply, but all are not agreed as to the particular point at which bodily heat should begin to be considered as an enemy to be dealt with separately and apart from the enteric disease. Abroad, where the cold water treatment of enteric fever has obtained a more general recognition than with us, it is common to begin with a temperature a little over 102° Fahr. For our own part, we hardly feel inclined to begin any specific treatment at such a line, but we claim no infallibility in the matter, and, in point of fact, may be wrong in so doing. Nevertheless it seems to us that temperatures under 104° Fahr. may as a rule be dealt with by increasing the quantity of alcohol given to the patient, by tepid sponging of the surface, and especially by the giving of quinine. As to the administration of alcohol, we hardly think it possible to lay down rules to be generally available. Sponging is simpler, and is available at anything over 100° Fahr. But to give quinine effectually it must be given in full dose; as a rule we begin with twenty grains, and continue for some time thereafter to give five grains every four hours. This, no doubt, gives some headache and buzzing in the ears, but that is only showing that the remedy is having the physiological effect which is essential to the due development of its power of depressing temperature. With these three remedies in our hands, not every case of typhoid requires cold baths; but if the temperature keeps rising, and remains persistently over 104° Fahr., then the bath should be brought to the bedside, carefully raised to the temperature of 70° Fahr., and the patient placed in it for ten minutes. This will cool the surface, but will have, in the first instance, little effect on the internal temperature, for causes on which we cannot now enter. After the patient has been replaced in bed, however, the internal heat also falls, but too often only for a time, inasmuch that in bad cases the baths may require to be repeated every two hours, and as many as 200 to be given in the course of the disease. Thus used there can be no question of the exceedingly great usefulness of cold water baths; the only diversity of opinion seems to be as to the proper time, as indicated by the patient's temperature, when to begin them. But even beyond this there are exceptions to ordinary rules. The effect of the cold water is to arrest or diminish cutaneous circulation, and consequently to produce some degree of internal congestion. If, therefore, there be threatening of intestinal perforation, when the necessary moving of the body might be dangerous, or of intestinal bleeding, when the congestion might give rise to troublesome hemorrhage, the cold water cannot

well be employed in this fashion; but it is not to be forgotten that the pulmonary congestion, so apt to be a dangerous concomitant of typhoid, is relieved rather than otherwise by the use of the bath.

We can hardly leave these complications, especially intestinal hemorrhage and perforation, without a word of warning. They are especially apt to occur when patients are making some extra exertion, such as the rising to stool or the like. These efforts, therefore, towards the period of the disease when they are most likely to occur—*i.e.*, about the third week—should be strictly forbidden. But this caution is also necessary in another way. Cases of typhoid do occur where the general symptoms are so slight that the patient will hardly consent to keep his bed. He may keep on his feet to the last, and suddenly perish of hemorrhage or perforation. Such risks would be avoided by regular observation of the bodily heat; but we mention them chiefly to enforce the rule, that as soon as a patient is known to have typhoid, he is to be sent to bed, and kept there till the temperature has been for some time normal both morning and evening.

Many other things might and perhaps ought to be said on the subject of treatment, but our space forbids. There is, however, one grand rule never to be lost sight of in dealing with this as with other diseases. Look after the little things. Good nursing is all-important in typhoid; and to insure good nursing the practitioner should always be a better nurse than the nurse herself—a kind of qualification which the rising generation of practitioners is too apt to sneer at. And then the diet; that, again, is a matter requiring profound consideration. If there is a question of a vessel or an ulcer giving way, a slight thing may do it; an over-distended bowel, whether from food or gas, may have a fatal issue. But of all things, careful dieting during early convalescence is most essential. Often a relapse is precipitated by the injudicious use of certain articles of food; and we have often seen the relapse from this cause much more severe than the primary attack.—*Med. Times and Gazette*, Aug. 8 and Oct. 10, 1874, pp. 148, 424.

3.—THE TREATMENT OF ENTERIC FEVER BY COLD BATHS.

By M. FRANTZ GLENARD, Interne des Hôpitaux, Lyons.
(Translated from the *Lyon Medical* by Dr. W. MACEWEN).

The starting point of Brand's treatment is a just appreciation of enteric symptoms and their therapeutical indications. In this way Brand, while throwing the blame of all the serious

complications and the fatal terminations of enteric on the prolonged excess of the febrile temperature, shows that there can be ranged, on one side the primary and necessary symptoms proper to the manifestation of enteric fever, pyrexia, elevation of temperature, bronchial catarrh, hypertrophy of the spleen, roseola, and various eruptions, signs which, with the exception of the temperature, are always mild, and which can neither be hindered nor extinguished by cold water; on the other side the accidental and consecutive symptoms, those dependent on the high temperature; cerebral and nervous symptoms—delirium, somnolence, coma, typhomania; serious affections of the pulmonary tissue—atelectasis, hypostasis, pneumonia, gangrene, ulcerations; intestinal lesions, fuliginosity, gastralgia, dyspepsia, intestinal catarrh, diarrhoea, tympanites, ulcerations and their consequences, hemorrhage and perforations; the lesions which Zenker has met with in the striped muscles, which Liebermeister has studied in the liver, the spleen, kidneys, and heart; the tendency to collapse, hemophilia, eschars, &c., &c., symptoms which in themselves constitute the gravity of enteric fever, and which are never met with in patients treated by cold water. But the danger of this elevation of temperature does not consist in the simple fact of its reaching a certain height, but in the maintenance of the temperature at a pernicious level. Even the temperature of 42° C. is not fatal if it fall soon to 39° C., but it soon becomes so if it remains long at that height; and this persistence presents the same degree of danger between 39° C. and 40° C. as between 40° C. and 41° C. This indication is then formulated: it is necessary to combat the excess of temperature during the whole course of the disease; that is to say, *to prevent exacerbations and maintain remissions.*

The first point in the treatment lies in its employment from the outset of the disease, for it is certain that refrigeration is more efficacious in preventing impending lesions than in curing them when the high temperature has produced a dissolution of the blood, or involved important organs. The phrase “from the outset” is to be understood as referring to the moment when the diagnosis of enteric fever is certain, or as soon as the physician is called. Precious time should not be lost in using drugs, and only resorting to cold baths when the case appears urgent.

Further, there is no harm in treating by this method febrile states that have an analogy to enteric; their evolution, in fact, is more rapid under this treatment than by the use of medicines. Only, it is necessary to guard against believing, in such a case, that cold water has arrested enteric fever. In the Saint-Pothin ward I have seen five cases of pyrexia assuming

an enteric appearance, that were cured after eight or ten baths, and I have not hesitated to eliminate them from my statistics though the symptoms at the beginning of the disease resembled those of enteric fever. Even when an error in diagnosis has led to the use of the baths in such a disease as acute tuberculosis, the effects are not unfavourable to the patient.

It would be most important if severe cases of enteric could be distinguished from mild at the outset. Those who oppose the system ask why inflict this barbarous treatment on 100 patients, of whom 80 will recover without cold water; but when this method has saved the 100, is it not culpable to refrain? There is no means of knowing that a mild case of enteric fever may not suddenly assume a most serious aspect. It is then too late. If the form is slight, so much the better, the patient recovers the sooner; if it is serious the recovery is certain, provided it is treated from the outset. If the treatment is applied later it is necessary to distinguish two categories: the first comprises cases which are submitted to cold water on account of the gravity of the type, and the insufficiency of the medicines: the second comprises serious cases in which grave complications have supervened. Of the first class the greatest number recover. Of the second it happens that a life is lost here and there from lesions which are necessarily fatal. It is not uncommon to have recourse to this treatment with a moribund patient; but Brand's method is not more efficacious than drugs against death.

The least important objections against this treatment are *difficulty* and *barbarity*. In civil hospitals the first question has long since been decided by the use of the baths in the practice of German physicians. In the Hotel-Dieu at Lyons the directorate of the hospital have arranged, in view of facilitating Brand's method, two wards for enteric patients, one for female, the other for male. The eleven cases treated in these wards up to this day have resulted in eleven recoveries. At the Croix Rousse Hospital the thirteen cases treated since mine have given thirteen recoveries.

In private practice, it is said, evidently without reflection, that the method is quite unsuited, as if the mere matter of convenience should present itself before that of the life of the patient. But here there is no difficulty; as I will show, further on, seventeen cases treated by cold baths in private practice resulting in seventeen recoveries.

Lastly, when everything has been considered, and it is agreed that the method is practicable, then the plea of its barbarity is advanced. This looks like the result of exaggerated sensibility, or want of consideration on the part of the physician. But even should the treatment appear barbarous (and

it will be shown that it is not), is all that accrues therefrom to be considered worthless; the certainty of the prognosis, the rapidity of the convalescence (three or twelve days in place of thirty or forty), the freedom from sloughs, and the sequent weakness of the intellect or the senses, the allaying of all anxiety on the part of the family or the physician? The physician cognisant of the treatment knows what power he has; and the family who see the patient rise on the third day to bathe himself, crying for food, serving himself, assisting in the change of his compress, &c., sleeping calmly, and, in a word, assuming the appearance of health, judge for themselves of the prognosis, and are satisfied.

1. The method of treatment which exercises the most favourable influence both on the progress and issue of enteric is that which considers the undue elevation of the temperature and the adynamic tendency of the disease, and which has for its principles of action *refrigeration* and *constant alimentation* of the patient.

Since Brand's work appeared, the statistics founded on 6 to 8000 enteric patients treated by this method, in Prussia, Austria, and Russia, give a mortality of 4.5 to 7.6 per cent., in place of 18 to 25 per cent., which used to result from the medicinal method.

2. The therapeutical proceeding which corresponds best with the indication: refrigeration consists—first, in the treatment by cold water from the outset of the disease, until it terminates in defervescence; second, to combat each exacerbation and maintain the remissions; third, to administer with that end large cold baths, repeated night and day (according to rules indicated above); cold compresses, cold lotions, and, finally, iced water to drink in the intervals of the baths. (Brand's process.)

That proposition is justified by our third conclusion:—

3. The aphorism of Brand, which says—*Every enteric patient regularly treated from the commencement by cold water will be free from complications, and will recover*, is true up to this date. It is the same for this aphorism: *Every degenerate case of enteric, where the treatment is applied late (after the first period), will present greater chances of recovery when treated by cold water regularly administered than under any other method of treatment.*

Out of our 47 cases, in fact, there have only been eight treated after the twelfth day. If the fatal case from Saint Pothin Ward be added, there would be eight successes out of nine cases; an eloquent result, if the peculiar conditions are taken into account, the treatment by cold water having been applied as a last hope when the prognosis was fatal, notwithstanding the use of the medicine employed.

The corollary follows. Every enteric patient treated by cold water who presents any complication, or who does not recover, has not been treated regularly and from the beginning. This is quite logical, and Brand should not reproach us for having formulated it.

It is to present the truth of this last proposition that we have separated the failures; for there will always be failures, suppose the method should be universally adopted, even in its detail. I only wish to show that cases which are suddenly struck down, and where the patient dies two or three days after he has been put to bed, are those either where the disease has been latent (*typhus ambulatorius*), or where the patient has been put to bed at the last extremity, and where the method having been administered, apparently at the commencement, will in reality have been applied when the enteric fever has reached its second period. The *post-mortem* will then reveal lesions ulterior to the tenth day (the soonest period, according to Griesinger and most authors, for the detersion of the intestinal ulcerations to appear), and will prove that the pathological commencement had preceded the subjective symptoms.

I do not require to add that such cases will present themselves very exceptionally in private practice, and that in the hospitals the mortality of enteric reduced to that minimum will not pass from 4 to 5 per cent.

Besides these conditions, in explaining the failures in the cases where the treatment has been regularly carried out from the commencement, there will require to be taken into account the cases of miliary tubercle, which are not so rarely mistaken for enteric as may be supposed.

As a final conclusion, the results obtained at Lyons by Brand's method to the treatment of enteric, justifies its adoption in the hospitals of that town, and encourages its use in private practice.

Description of Brand's Method.—According to the indications, the physician has the selection of any method which produces refrigeration: Cold compresses, the shower, various lotions, &c.; their regular and methodical application being the only condition indispensable to success. Circumstances and social position may regulate the choice of the refrigerant. Brand prefers the plunge bath, which is the most suitable, its action not depending on the zeal of the nurse. The water should be at the same temperature throughout, and it should not be a tepid bath gradually cooled, the shock in the former case being regarded as beneficial. The instructions to the nurse are as follows:—The nurse will, every three hours, take the rectal temperature of the patient, and give him a bath at

20° C., of 15 minutes' duration, night and day, until the thermometer placed in the rectum for five minutes does not register 38·5° C. The patient is taken to the bath, his night dress removed, and he is plunged *up to the neck* in the water, at 20° C., while the head is sprayed with water at 6° to 8°—an important detail, especially where the patient presents cerebral symptoms. After the nervous symptoms have been allayed, the spray may be given at the same temperature as the bath. That affusion having lasted one or two minutes, the nurse rubs the limbs of the patient for three or four minutes; then he is left at rest. His breathing may become difficult, and his teeth may chatter, but he must remain in the bath for 15 minutes. When he is about to be removed from the bath the affusion is to be repeated. He should be kept at least 15 minutes in the bath, even should the shivering set in from the commencement; and longer should the shivering be late of appearing.

He is then removed, his night dress put on, without drying him, a sheet is thrown over his feet; his mattress should be hard enough not to yield to the weight of the body, which should be covered by a sheet in summer (also by a light linen cover in winter). A little weak tepid soup is now administered, along with a mouthful of old wine, and he is left alone to rally from his shivering, which lasts 15 to 20 minutes, sometimes for even an hour.

Fluid nourishment should be given regularly, and always tepid—mouthfuls of iced water should be taken from time to time. If the patient is very weak, a spoonfull of old wine may be administered before the bath.—*Glasgow Medical Journal*, July, 1874, p. 370.

4.—ON THE EXCITING CAUSE OF ENTERIC FEVER.

By Dr. WILLIAM STRANGE, Senior Physician to the Worcester Infirmary, and Medical Officer of Health to the City.

In ninety-nine cases out of every hundred of enteric fever the origin is not from any germ or contagium derived from the intestines of some person ill of the same disease, but pythogenic—that is, bred of a poison generated in decomposing or putrid animal matter, chiefly when dissolved or suspended in water; and which, exhaling into the atmosphere, enters the lungs or is taken into the stomach in drinking-water. There, possibly, it multiplies and generates a true contagium, which, when it is added to sewage matters, or to drinking-water—*especially if the latter be also foul*,—becomes a still more virulent poison, or else the same poison in a more active state than that which, generated pythogenically, first induced the fever.

Very many years ago, in a school under my cognisance in a small country town, twenty-six pupils, out of a total of about sixty-five, were attacked by enteric fever, all within a few days of each other. The school was dismissed. On reassembling the next half, it was not found (although the evidence of this could not be considered very exact) that any-one, nurses or friends, except the boys actually at the school, took the disorder. The cause of the outbreak was a series of very filthy privies in which the boys were in the habit of congregating. These privies were totally unconnected with any drain, and no one, except the boys, was in the habit of using them, nor was there, nor had there been, any case of the fever in the town or neighbourhood for years. The curious thing was that the fever was generated at the beginning of a half-year, when the contents of the privies had been undisturbed, and had had time to get stale, and for ulterior changes to take place in them.

In the year 1869 a violent outbreak of enteric fever took place at a healthy village near this city. In a row of cottages, about twenty-eight in number, there were upwards of twenty cases, and several deaths. There was no drainage attached to the cottages; they had, for about every two houses, a privy and cess-pit. These privies becoming foul, the owner of the cottages had them all attended to at one time, the contents being thrown into a heap at the back of the cottages, and allowed to remain for some days in exceedingly hot weather. There was no drain or sewer, nor other means by which the enteric contagium could have passed from house to house, and no person ill of the disease had been known to arrive there.

If it be said that some person affected with the disease might have used one of these privies, and that the contagious germs from his intestines had thus had an opportunity of exhaling into the atmosphere when the night-soil was disturbed, and so infecting the inhabitants, how is it that, when we have these very germs bodily in the bed-pan in the sick-chamber, they do not exhale in like manner, and infect the nurses and friends of the patient? Is it not far easier to believe that the poison was here generated *de novo* by the action of the sun and atmosphere upon this putrid mass of fæcal matter; and as the mass exposed was great, so the quantity and virulence of the resulting poison was great?

But, whichever theory may best explain the circumstances of this outbreak, that the poison was volatile and existed in the atmosphere was positively certain from the following fact. At the time this outbreak was taking place, I received into the Infirmary a servant-girl suffering from enteric fever, who had been living with a family in a fine open square of this

city. On questioning the girl, I found that she had visited this village for the purpose of attending the funeral of her father, who had died of the disease. I ascertained that she had remained in her late father's house and in the open air adjoining for about two hours. She did not see her father, the coffin having been screwed down when she arrived. She neither ate nor drank during her visit; but she perceived in the house, and in the neighbourhood of it, a most sickening smell, which she could not get rid of after her return home. If this girl's attack, which lasted six weeks before defervescence was fairly established, were owing to germs or a contagium floating in the atmosphere, and derived *directly* from the intestines of some other person ill of the disease, how volatile and all-pervading must that contagium be! Yet do we find that it is so? Indeed, Dr. Corfield thinks it may be not more so than a solution of acetate of lead!

But a crucial test is better than probability, even when as strong as that derived from the consideration of such instances as those just adduced, and which might be multiplied by the score out of the experience of those who have seen or observed much of enteric fever in the country. At a large country house at which I once lived, three servants—two living in the house, and one out—were in rapid succession taken ill with undoubted enteric fever. For some time no cause could be discovered, as the house was situate a mile from any other dwelling, large, well drained, and well ventilated, and well supplied with water. Whilst the servants were ill, some of the household complained of the nasty taste of the drinking-water, which was brought to the house from a fine spring on the estate in leaden pipes, and stored in a leaden cistern. The water was noted for its purity throughout the neighbourhood. On examining the cistern several rats were found in it in a state of decomposition, and the atmosphere over the cistern, in a close situation, smelt distinctly of putrefaction. The fact came out that the servants had used arsenic spread on bread and butter for the destruction of the rats, which had resorted to the cistern to allay the thirst caused by arsenical poisoning, and so got drowned. On clearing out the cistern, the illness ceased.

That *rat broth* can thus give rise to enteric fever must, I think, be admitted; and if an infusion of rat, why not that of any other animal, or of animal matter, whether in cisterns, wells, drains, sewers, or ponds—*e.g.*, farm-houses and the cottages about them, which are the favourite habitat of enteric fever?

It would be inexcusable to take up the valuable space of this journal by multiplying instances like those few I have men-

tioned. One meets every day country practitioners who say that they would believe the exclusive contagium theory of the causation of enteric fever *if they could*, but that the facts are against it; and facts *are* stubborn things, which, when we cannot conquer, we sometimes are fain to evade. But one thing I may say—that is, that had the exclusively contagious theory of this fever been universally recognised as the one and sole cause, so little faith have I in the stability of medical theories as against the observations of the senses, that I would have thrown that theory over unhesitatingly on the occurrence of the rat fever above described. For, after all, theories must be brought to the bar of experience for confirmation or dismissal. The busy practitioner, who has read little of the opinions of others, will be apt to form a strong conclusion from the results of his own observation; whilst the bookish man, without practical experience, on the other hand, will weigh the evidence afforded by a multitude of writers, and probably draw conclusions which so soon as he has had the opportunity of personal observation, he will repudiate.

Now, there is no readier method of falling into error than by coming to conclusions on the faith of analogies (so-called), for analogies in medicine seldom hold beyond certain points of resemblance. If we take these points of resemblance, and generalise upon them, without correcting our conclusions by their contrasts and opposites, we shall commit what is a very common error indeed, but a very great one. Dr. Corfield and those who agree with him make much of the analogies between enteric fever and others of the zymotic class—viz., small-pox, scarlatina, *et hoc genus omne*. They call enteric fever an exanthem of the intestines. Are there not, they ask, the rash, the febrile condition, the high temperature, the definite duration (?), the affection occurring but once in a lifetime, and one attack preserving against another? These are the analogies. Now for the conclusions drawn from them. Small-pox and its fellows are propagated by contagion solely, by special poisons imbibed into and then multiplied in the body, and never—in these days, at least—arising *de novo*, as that would be contrary to the canon of ratiocination, which does not permit of our assigning a double mode of causation when one will suffice to account for the phenomena. Therefore, enteric fever, the analogue of small-pox, is communicated by a special poison, multiplied in the intestines, and carried thence into the system of others, who are never attacked except through the means of this intermediate propagator. Then there is the analogy of the rash. But this rash is often absent, and when present it by no means bears any constant proportion to the severity of the fever. The

febrile condition and high temperature must be granted, for at least these diseases are all *fevers*. But what of the definite duration? I have seen enteric fever, whose febrile period is generally some eighteen or twenty-one days, lasting four, five, or six weeks without marked defervescence, or, if this have occurred, the high temperature easily re-induced, and maintained for a period equal to that of the first accession. Now, this does not happen in the case of small-pox or scarlatina. In those it is the *sequelæ*, not the original febrile symptoms, which show such chronicity.

Then the affection of the mucous glands of the intestines has been likened to the exanthema of variola, &c. But in many cases we have but slight evidence of a serious affection of those bodies, nor can their ulceration be taken, without a great strain, to have more than a very distant resemblance to the rash of measles or the pustules of variola. Nor can the fact of typhoid occurring, as a rule, only once in life prove anything more than that, *as a fever*, it has that character with some others, but not which all, typhus being clearly an exception.

I will glance now at the distinctions and oppositions of character between enteric fever and the exanthemata in regard to the mode of action of its contagium, supposing there to be one. The contagious principles which generate small-pox, scarlatina, measles, &c., are, without doubt, solid, portable substances, exhaled from the person of the sick, and carried about by clothing or other substances, whence they are imbibed by the skin or lungs of the recipient. But the *materies morbi* of enteric fever does not act thus. The nurses and attendants upon the sick neither imbibe it by the skin nor inhale it by the lungs. The advocates of the contagium theory assume that it is too heavy for this. They admit, also, that, to become active, it must either have been added to *fæcal* or other putrid animal matters, or to drinking-water—which, by the way, is generally polluted with sewage-matter. Even given these conditions, the existence of the special poison is generally only inferred from its effects; and, although I by no means deny the existence of such a contagium, many will say that most of the cases, where the action of a specific poison is presumed, may be more easily accounted for on the pythogenic theory.

The only logical conclusion is that enteric fever is a disease having certain resemblances to the pure exanthemata—small-pox and the rest; but with differences which are specific, if not generic. Amongst these may be mentioned the lengthy and very various prodromata, the uncertain duration and

frequent recurrence of the febrile symptoms, and the want of harmony between the severity of the general symptoms and that of the intestinal lesion.

As to the exciting cause, authorities of equal weight, and equally modern, attach different degrees of importance to certain observed facts, the only mode of reconciling which will be found in acknowledging *at least* a twofold origin and propagation of the disease in question, or, rather, of a primary and a secondary cause. And I think it is a fact that the witnesses to the generality of the pythogenic, or, as we say, the *de novo* origin of the disease, very largely outnumber those who pin their faith to an exclusively contagious origin. The larger number affirm that it is impossible to ignore, for the sake of a taking theory, the evidence of their senses; the smaller number say, We can *prove* the presence of an active contagium in *some* cases; we believe it to be present in all. *Voilà tout !*

One word on the bearing of this question upon the action to be taken and the advice to be given by medical officers of health in the discharge of their very responsible duties in regard to the prophylaxis of enteric fever. How can I—who have never, in any single case occurring in this city which has come under my cognisance during twenty years, been able distinctly to trace it to the operation of a contagious element—be content with recommending people to take especial care to disinfect and destroy the discharges emanating from any patient who may have enteric fever? To lay the chief stress upon this mode of propagating the disease would be to reduce the usual pythogenic action of fæcal matter to less than its due importance. I am bound, therefore, whilst not neglecting to see that the discharges are properly dealt with, to enforce every precaution against the pythogenic origin of the disease. These precautions I take to be the disuse, as far as practicable, of all well-water in thickly populated places; the ventilation of all sewers, drains, privies, and water-closets, and the shutting off of the gases contained in any of these structures from entering any dwelling-house or work-room; and, generally, of preventing the lodgment of *any fæcal or other animal matter for more than a few days* in any such places; and, lastly, to make war to the death against the hideous privy and ash-pit system, which is the chief means of carrying pestilence into the atmosphere of our towns, and the main cause of that excessive amount of infant mortality by which they are disgraced. And this course I would take the liberty of recommending for adoption to my brother medical officers of health. —*Medical Times and Gazette*, June 20, 1874, p. 667.

5.—TYPHOID FEVER THE RESULT OF INSANITARY CONDITIONS.

By Dr. ANGUS MACKINTOSH, Medical Officer of Health,
Chesterfield.

As all professional men are not yet agreed on the real cause of typhoid fever and its *modus operandi*, it will be interesting to know any new circumstances that may favour or retard the poison or infection of this type of fever. I have reported elsewhere my belief that typhoid fever in every case is the result of a specific poison being introduced into the system, either through the medium of air or water, but I have yet to be convinced that the virus which causes this condition must be conveyed from one typhoid patient to another. The following case, which happened in my district ten weeks ago, is, in my opinion, an extremely strong one against the latter theory, and consequently worthy of being recorded.

About the end of December, 1873, a collier, with wife and four children, moved from the parish of Great Barlow, in the Chesterfield Union, to Long Row, Northwingfield, in the same Union, a distance of eight miles. Great Barlow is a rural parish and thinly populated: the population in 1871 was 959. So far as known to me, there has been no fatal case of typhoid in the parish for years, and none fatal or otherwise for the last nine months, during which period I have been acting as medical officer of health for the district. The house in which this man lived was an isolated one, and the conditions surrounding it, in a sanitary point of view, were very fair, so that, so far as can be traced, there was no chance of any poison or infection being conveyed with the man, wife, or family from their former home.

Long Row, to which the man, &c., went, consists of about thirty houses, and is also in a rural parish. Scarlet fever and measles have been lurking about the place more or less for the last six months, but there has been no case of typhoid or enteric fever in the Row for a considerable time. The house this man chose for his residence is about the middle of the Row, was previously occupied by a healthy family, and, so far as is known, there has been no enteric fever in it. The newly-arrived family used the same drinking-water as the rest of the inhabitants, which is procured from a well in a field about a quarter of a mile distant, and similar insanitary conditions as to sewage emanations surround each house, the privies and ash-pits being at the same distance from every door in the Row. The drainage system, which is very imperfect, consists of an open surface-drain in front, and a trapped drain two yards from each house behind. The houses are of modern construction, and have through ventilation.

The new-comer was only about three weeks in this place when three of his boys were attacked with typhoid fever; the youngest, eight years old, was the first seized, then the other brothers; then their sister of twelve; after a few days the mother, and a fortnight later the father. Not one in the house escaped, and up to this date, when the family is quite convalescent, no other resident in the hamlet has caught the infection. Every precaution was adopted, and plenty of disinfectants freely used.

These facts would go to prove the following conclusions:—

1. That the family was free of the infection of typhoid fever when they came to the Long Row.

2. That the constant exposure to sewage emanations appears to protect the system in some degree against the effects of typhoid virus.

3. That, in some instances, individuals after a change of residence from a healthy locality to one favourable to typhoid fever are more susceptible to the poison than those long resident in it.

4. That the sewage emanations in this place were no doubt the cause of this attack, and that the stage of incubation was from fifteen to twenty days.

5. That sewage emanations are sufficient to create and produce typhoid fever, though not contaminated with the discharge of typhoid patients.

6. That unless the virus was totally destroyed by means of the regular system of disinfection carried out, this case would not go to prove the contagious nature of typhoid fever.

I may add, that since the beginning of 1874 I have had twenty cases of typhoid fever in my district, and not one of them could I trace to the presence of typhoid discharges in the water, or the effluvium proceeding from typhoid evacuations; and, what is very singular, two cases have not occurred in the same place (with the exceptions of the family above-mentioned); they were sprinkled over the district, and almost the whole were associated with sewage emanations and organic decomposition, and in some cases polluted water.—*Lancet*, May 9, 1874, p. 657.

6.—ON TYPHUS FEVER.

By Dr. THOMAS B. PEACOCK, Senior Physician to St. Thomas's Hospital.

[Typhus fever is a disease which occurs more particularly in persons of middle and advanced age; its incubative or latent period lasts from ten to fourteen days.]

The eruption usually appears as small discrete spots, slightly elevated, of a dingy red colour and fading on pressure. In a short time the spots cease to be elevated, and fade less completely on pressure, and a purple mottling appears in the interjacent portions of skin. At a still later period—say on the eighth, ninth or tenth day, the spots become entirely petechial, not being at all affected by pressure. Sometimes the eruption which first appears is more discrete and more decidedly elevated, so as to bear for the time a close resemblance to the typhoid rash, but when the general mottling appears there can be no difficulty in recognising the nature of the eruption. The appearance of the rash also varies according to the condition of the patients. In young and healthy persons it is usually of a more pinky hue and continues to fade on pressure nearly the whole of the time it is out, while in debilitated persons and those previously out of health or in the aged, the spots are very livid and soon cease to fade on pressure, or they may from the first be petechial, and sometimes they form large purple blotches. In young people the eruption is often very peculiar—it assumes the form of small distinctly rounded purple spots, very closely resembling fleabites, but which under the lens are readily recognised by the absence of the central punctum. The eruption also varies considerably in its diffusion and in the period during which it remains. Sometimes it is only slight, and is chiefly seen on the abdomen and thorax; in other cases it covers very thickly all parts of the trunk and extends to the neck, shoulders, upper arms, and thighs, and more rarely to the hands and feet. I have also occasionally observed some spots on the face. When the eruption is only slight and pale it may also be very transient, continuing for a day or two or being visible only for a short time after the patient has been in the bath; when, on the contrary, it is copious and soon becomes decidedly petechial, it is of long duration, continuing even for ten days or a fortnight or to near the end of the third week. In two cases, that of a mother and child which have recently been in the hospital, the rash has been very peculiar in its character. In the case of the mother there were only a few purple and rounded spots about the upper part of the thorax and on the neck when the patient was first seen, the period of attack not having been precisely ascertained, and the nature of the case was by no means clear, though the rash was regarded as probably typhus passing away. In the child there was a copious crop of spots, quite similar in character to those in the mother, over the thorax and abdomen. In both cases the fever was complicated by severe pulmonary symptoms, and the convalescence has been very protracted, probably in consequence of the patients having been in a very destitute

condition for some time before the commencement of the attacks.

The time of disappearance of the eruption was observed in seven out of the ten cases in which it was present at the time of the patients' admission, and in these it continued out for 4 days, 6 days, 7 days (3 cases), 10 days, and 12 days.

In the cases in which it was apparently coming out at the time the patient was first seen, it continued 8 days, 9 days (2 cases), 11 days (2 cases), and 13 days; and in the cases in which it came out while the patients were under observation it disappeared in 4 days, 8 days, 10 days, and 11 days. Usually as the eruption fades the patient's condition improves, and convalescence is generally fully established in from three or four days to a week after it has entirely disappeared. In cases, however, where the patient has been out of health previously, in which the prostration of strength has been extreme, or where there have been serious local complications, such as bronchitis or pneumonia, bed-sores and sloughing of dependent parts, abscesses or erysipelas, the convalescence may be very protracted. Thus, in the cases in which the eruption was supposed to be coming out it appeared on the second day of illness, faded on the eleventh, and convalescence was established on the fourteenth day in one case. In others—

It appeared on the	Faded on the	Convalescence was established on the
5th day . . .	13th day . . .	29th day
6th „ . . .	17th „ . . .	21st „
7th „ . . .	16th „ . . .	24th „
7th (worse one day) .	20th „ . . .	37th „
8th or 9th day . . .	19th „ . . .	29th „

In the cases in which the eruption came out while the patients were in the wards—

It appeared on the	Faded on the	Convalescence was established on the
4th day . . .	15th day . . .	26th day
4th „ . . .	14th „ . . .	17th „
5th „ . . .	15th „ . . .	33rd „

In the case in which it was first noted on the eighth day the time at which it disappeared is not named in the report.

In the other symptoms which usually characterise typhus there was nothing unusual in this series of cases, except that in several instances the bowels were much relaxed. Usually the bowels are confined during the course of the disease, but such is not always the case; sometimes towards the height of the fever and when there is great prostration of strength, the bowels being relaxed, apparently from want of power in the sphincter to retain the fecal matter; but in several of the

cases now reported upon there was profuse diarrhoea during the whole course of the disease, and this independently of any medicine having been given. Indeed, it is quite impossible to base the diagnosis between typhus and typhoid upon the confined state of the bowels in the former disease and the occurrence of relaxation in the latter, for, as I shall have again occasion to remark, it is not very uncommon for the bowels to be confined in typhoid. So also, though, as a rule, the cerebral disturbance is more marked in cases of typhus than in typhoid, it sometimes happens that a patient will pass through a marked or even severe attack of typhus without much delirium, and retaining his intelligence to such an extent as to be able to answer simple questions put to him without any apparent difficulty. In such cases, however, the patient, on recovery, has no recollection of anything that has occurred from a very early period of his illness till convalescence is far advanced.

The general course of a case of typhus may be said to be, that the patient is suddenly seized and the disease rapidly progresses, so that he applies for admission on or before the seventh day of illness, when he is usually so prostrated as to require to be carried to bed. The eruption on the skin is then generally out, but in cases seen at an earlier period it is observed to make its appearance on the third or fourth day from the commencement of the symptoms, and it generally attains its height on or about the eighth day, when the active febrile symptoms will also be most marked, the temperature rising to 103° , 104° , or even to 104.5° . The eruption begins to fade about the ninth or tenth day, and disappears about the fourteenth, and, if there be no local complication and the patient has not been very greatly prostrated, convalescence is established between that day and the twenty-first. In slighter cases, however, the serious illness may continue for only about a week, the eruption may never be very marked, and the patient may become convalescent from the tenth to the fourteenth day; while, in very severe cases, the rash may become petechial at an early period, and may continue on the skin till near the end of the third week, and the convalescence may be very greatly protracted. Generally, in a simple uncomplicated case of typhus, the pulse and temperature fall below the normal standard at the earlier period of convalescence, and again rise when the patient takes more food and is capable of some little muscular exertion; and usually till recovery is well advanced, any slight cause, such as cold, will be attended with a very marked increase of frequency in the pulse and a renewed elevation of temperature. If death occur it is generally towards the middle or end of the second week or at the early part of the third week.—*St. Thomas's Hosp. Reports*, 1873, p. 5.

7.—ON THE ORIGIN AND PROPAGATION OF TYPHUS FEVER.

By Dr. J.-F. GUILLEMIN, Médecin-Major des Hôpitaux Militaires, Paris.

From the fact that the origin of some cases and some epidemics of typhus fever has been traced to contagion, it has been by many most illogically concluded that all cases and all epidemics of the disease in question must be due to the same cause. In the interesting monograph before us, Dr. Guillemin has presented to us very important facts bearing upon this question—facts which, we believe, establish the doctrine that typhus fever may arise spontaneously in certain well-known unsanitary conditions; and that, in fact, in several remarkable instances it has actually arisen so. Dr. Guillemin commences by contesting the opinion of Professor Chauffard, expressing in a communication to the Academy of Medicine on October 15, 1872. This last-mentioned writer, reviewing the circumstances of the civil and military population in Paris and Metz during the late war, maintained that all the generally admitted causes of typhus epidemics existed in these towns in full force, and that notwithstanding there was no typhus. From this M. Chauffard not only denies the possibility of a spontaneous origin of typhus in France, but generally the possibility of the spontaneous origin of typhus epidemics. It would be sufficient, in opposition to this (says Dr. Guillemin), to show “either that the conditions which render the spontaneous explosion of typhus inevitable neither existed at Paris nor at Metz, or that cases of typhus were observed in one or other of these two towns. One is not permitted,” he continues, “in order to solve the question of the origin of an epidemic disease, to limit one’s self to a study of the circumstances in which it has not appeared; for, even supposing M. Chauffard’s assertions perfectly established—that is to say, first, that typhus was not observed either at Paris or at Metz; and, second, that all the conditions generally recognised as leading to its production were found combined there—that by no means authorises us to infer the necessity of importation. We may with as much reason conclude that, if the action of the habitual causes of typhus have not produced its ordinary effects, this is because it has been trammelled by circumstances which have escaped observation, and which ought to be sought after.” Dr. Guillemin maintains that the absence of typhus from Paris and Metz, even if true, which it was not, would not justify M. Chauffard’s conclusions. It would have been necessary, in order to do this, says the author, “to pass in review the epidemics of typhus upon the origin of which we have positive

information, to study the conditions in which they were produced, and to show that importation has been the condition *sine quâ non* of their appearance. This has not been done; indeed, it could not be done, seeing that there is a large number of epidemics of typhus in regard to which importation is in no way admissible."

Passing in review the work of Pringle, whom our author quotes, as supporting the spontaneous theory of the origin of typhus in individuals and communities placed in conditions of organic deterioration, subject to overcrowding and general unsanitary conditions, he proceeds to show how these causes gave rise to typhus in the French and English armies in the Crimea.

The French troops were forwarded to the Crimea in September, 1852, and the first typhus epidemic appeared in the following December, at a time when the severe cold condemned the soldiers to suffer from overcrowding and deficient ventilation. Félix Jacquot thus graphically describes the condition of the French. "After a prolonged stay in the mud of the trenches, after sentry duty, general labour, road-making, marches in deeply cut up and sinking fields, after having been soaked by rain and snow, the soldiers shivering, and very often wanting in changes of raiment, crowd themselves under the tents and huts, light if they can a meagre fire, and close hermetically all the openings with a perseverance and pertinacity against which the most pressing counsels and the most severe measures fail. The extreme uncleanness of the men, the foetid expirations, tobacco smoke, the evaporation from the wet clothing, all unite to make these narrow holes pestilential. Within is typhus, without is frost-bite, pushed often to complete sphacelus of the feet. The danger appears everywhere, but the worst is within."

Almost at the same time typhus appeared in the English and Russian armies in the Crimea, and in the Russian and Turkish armies in Asia. At the end of 1855, the conditions which we have mentioned persisting with even increased force, a second epidemic of typhus broke out in the month of December, and raged with greater violence than the first. The weather was extremely cold, the thermometer falling sometimes to 7.5° or 11° below zero Fahr. The overcrowding was excessive, ambulances originally constructed for 200 or 400 men often containing twice or three times that number. It may be said, says Dr. Guillemin, that these circumstances only favoured the transmission of the disease to a large number of persons, and that the second epidemic was due to sporadic cases occurring from the time of the decline of the first. "This opinion," continues our author "becomes very difficult to defend if we compare the sanitary state of our army with that of the Eng-

lish army at the same time. During the first year of the war the English, for a long period unaccustomed to war, did not know in time how to take proper measures to place their army in the best possible hygienic condition; it was as badly nourished, as badly lodged, as ours; it was decimated by typhus. But, contrary to what happened with us, the English knew how to profit by the rude lessons of experience; they understood that prevention was more easy than cure; after having recognised the cause of the evil, they did not hesitate to make the necessary sacrifices to suppress it; their soldiers were lodged in well-constructed, well-aired and well-heated barracks; there was an abundance of good food, and measures were taken to avoid overcrowding. What happened? The English army almost completely escaped the second epidemic, which affected the French army to a greater extent than the first.

“Is not this experiment on a large scale the most striking demonstration which could possibly be given of the spontaneous origin of typhus? The two armies are placed side by side on the same soil; the one, badly fed, placed in deplorable conditions, subjected to the influence of overcrowding pushed to its most extreme limits, and counting in its ranks a large number of men exhausted by intestinal affections, scorbutus, and anæmia, is a prey to typhus; the other, well fed, placed in good hygienic conditions, provided with ambulances and hospitals in which the greatest care is taken to prevent overcrowding, is spared by typhus.”

In 1861 MM. Léonard and Marit reported upon a severe epidemic of typhus which appeared in two villages in Algeria. The epidemic occurred in the month of February, and was strictly confined to these two villages, which were noted for their entire disregard of sanitary requirements. The houses, or rather hovels, consisted of a ground floor almost level with the soil; there were no windows, the only openings being the doors; the lanes and courts served as so many dépôts for dirt and every kind of excrement. Each of these holes would contain from eight to nine persons, with certain domestic animals in addition. The bare humid soil, tainted with ordure, formed the family couch. “Here, then,” says Dr. Guillemin, “is an epidemic with which importation had nothing to do. The importation could have taken place in but two ways: either the typhus had gained these two villages by propagating itself gradually from one locality to another—and then its tract could have been followed, inasmuch as it would have affected other populations before reaching the heart of Kabylie. Now the epidemic was quite circumscribed and perfectly localised in the two villages of Seddouck and Immoula; it had not been noted previously in any of the neighbouring villages. Or the typhus

had been imported by isolated individuals coming from some locality more or less distant where it existed at the same time; this supposition is as inadmissible as the first, for typhus at that time had not been observed in any other part of Algeria."

Dr. Guillemin says that the epidemic of Kabylie proves that importation is not so easy as some imagine; inasmuch as, notwithstanding that no precautions were taken to prevent importation or exportation, the surrounding villages remained free from the disease.

In the typhus epidemic which appeared in Algeria in 1868, the same unsanitary conditions which prevailed in the Crimea existed in full force. The first cases in general came either from overcrowded prisons and asylums, or from ambulances and hospitals into which the native sick had been admitted. In the spring of 1863, however, typhus had appeared in the town of Constantine, and continued there in a sporadic form "*pendant les années suivantes.*" It was, so to speak, acclimatised there, so that the first notion of some physicians was to attribute the epidemic of 1868 to contagion from that of 1863. This view might be maintained if one ignored the fact that at the time ("*meme moment*") when the disease exploded at Constantine it showed itself equally in a great number of localities of the two other provinces of Algeria, "where typhus had never been observed till then either in the epidemic or the sporadic state." For, at the time when the epidemic broke out in Constantine, it showed itself at Orléansville, Oran, Tlemcen, Mascara, Bel-Abbés, &c., localities very remote from the first, and which had no direct communication with it. Under these circumstances, one is compelled to admit that no relation could be established between the endemic of Constantine in 1863 and the epidemic of 1868.

In the second part of his monograph, Dr. Guillemin treats at length of the formation of typhus centres, of which he says, the chief causes are overcrowding, especially overcrowding of the sick and wounded, uncleanness, starvation, and the association of persons suffering from diarrhoea, dysentery, erysipelas, gangrene, &c.; and certain facts authorise him to state that this may occur in the open air. In this division of his work he mentions that in the penitentiary of Aïn-el-Bey the first case of typhus showed itself in an infirmiry assistant, whose duty it was to dress suppurating wounds, at a time when there was not a single case of typhus in the penitentiary.

Speaking of Metz, Dr. Guillemin maintains, contrary to the received opinion, that there were undoubted cases of typhus in that town, and that, if it had not capitulated, these cases would have reached the dimensions of an epidemic. But the condition of Metz was by no means very bad. As to food, "without doubt

it was deficient, but those who have not looked into the matter closely form a false idea of the extent of this deficiency; they forget that butcher's meat never failed, and that the daily allowance of it had been even largely increased, since '*dans les derniers temps*' it reached 750 grammes, *i.e.* nearly two pounds; it is true it was horseflesh, but this remained during the whole time of sufficient quantity." Dr. Guillemin informs us that bread only became scarce about fifteen days before the capitulation. At best then it was of passable quality, and, although diminished in quantity, strictly sufficient. Only in the two or three last days distribution of bread had entirely ceased, and then only the soldiers begun to suffer from hunger, although they still had meat.

"Many among them were very feeble, it is true, but they were not cachectic; no one had observed among them scurvy, as in the Crimea, nor that feeble condition of body due to starvation; there was neither epidemic diarrhoea nor dysentery. To sum up, in the army camped round the town there were no encumbrances, no great fatigue, no antecedent disease, and, consequently, no organic deteriorations; there was, indeed, insufficiency of food, but only for a fortnight or three weeks at most."

In the last chapter of the second part of Dr. Guillemin's paper, he refers to the spontaneous origin of typhus in France, and gives several instances, such as the epidemic in the convict prison of Toulon in 1830; that of Strasburg prison in 1854; that of the prison of Nancy in 1854-55, &c. In none of these cases could importation be traced, "although," says our author, "one may always, it is true, to explain the appearance of an epidemic, allege importation even when it is not possible to prove it; it is easy to say that the importation has passed unperceived; this is a convenient proceeding, but not a serious argument; importation ought, in my opinion, to be rejected when it is neither proved nor probable."

From these statements we think it may be fairly concluded, that typhus is a disease which may arise spontaneously under certain well-known conditions; and whenever these conditions exist in sufficient force. The assertion that the origin of the disease is always due to contagion is a pure assumption, for unless it be eternal, which no one has as yet affirmed it to be, it must have commenced some time; and the conditions which gave rise to its first appearance may have given rise to some of its subsequent appearances. And there is not only no proof that these conditions are now non-existent, but much that they are existent, and that they appear and disappear at certain periods, and always under the same or similar conditions. Indeed, to such an extent is this true that epidemics of typhus

may be predicted, as Dr. Murchison says, "with tolerable certainty," or, as Jacquot says, "typhus may be produced at will." "If," says Dr. Murchison, "in the case of every epidemic of typhus the first patient has contracted the disease from a person formerly affected, although it is impossible to trace the source, the contagion must needs be most potent and indestructible, which is the very reverse of the truth. The poison of typhus requires neither heat nor disinfectants for its destruction, but at once becomes inert on free admixture with fresh air. The opponents of the independent origin of typhus, in order to account for certain epidemics, are under the necessity of contending that the contagion exists in every part of the world ready to manifest itself under circumstances of overcrowding and under no other conditions; but for all practical purposes this is begging the entire question. If the poison remain passive for years so long as certain conditions are absent, but becomes active or potent so soon as these conditions come into play, it seems fair to infer that the appreciable conditions, and not an omnipresent and indestructible poison, constitutes the primary cause of typhus."—*London Med. Record*, Sept. 23, 1874, p. 593.

8.—ON THE VALUE OF GUARANA IN VARIOUS FORMS OF CHRONIC RHEUMATISM.

By Dr. EDWARD A. RAWSON, Assistant Surgeon to the Carlow Infirmary.

Suffering severely from lumbago, and finding all vaunted remedies fail, I tried guarana as an experiment. I took 15 grs. blended with hot water, and added cream and sugar. For twenty-four hours afterwards I had a delightful relief from pain. I thought it must be a coincidence; but, on a return of the lumbago, took another dose in the same manner and with a similar result. I gradually increased the dose to 40 grs., and took it regularly, once a day, for about a week. The lumbago disappeared. I gave up the guarana, and in a few days the pain in the back returned. A 40 gr. dose removed it, and it did not return for several days afterwards. Now, whenever it does, I have my remedy at hand. During the last month I have experimented largely with guarana on a variety of patients, rich and poor. The results vary. When the pain is acute, coming on with sharp stings, guarana acts like magic; when it is of a dull, aching character, the drug is slower in its action, and several doses must be taken before any decided benefit can be perceived.

I have come to the following conclusion, viz., that whenever the fibrous envelopes of nerves, the aponeurotic sheath of

muscles, the fasciæ or tendons are the parts affected, guarana gives if not instantaneous, at least very immediate relief, which will last from twelve to twenty-four hours; and I confidently expect that perseverance in the use of the drug, gradually increasing the dose up to 40 grs., will entirely remove any of the above mentioned kinds of rheumatism.

Of the good effects of guarana on nervous hemicrania there is no doubt; and I trust it will prove, in other hands, as valuable against rheumatism as it has in mine.

I find guarana was examined by Martins in 1829, and by Gravelle in 1840, According to them "it stimulates, and at the same time soothes, the gastric system of nerves, and reduces the excited sensibility of the coeliac plexus, thereby diminishing febrile action, and strengthening the stomach and intestines, particularly restraining any excessive mucous discharges; at the same time increasing the action of the heart and arteries, and promoting diaphoresis."—*Irish Hospital Gazette*, April 15, 1874, p. 120.



9.—TREATMENT OF ACUTE RHEUMATISM BY IMMOVABLE BANDAGES.

Dr. OEHME reports the observations made by Heubner in the hospital at Leipzig, on the treatment of acute rheumatism by fixing the parts immovably. Similar experiments had already been made by Seutin and Gottschalk; and in more recent times by Concato of Bologna, with good result. Heubner used pasteboard; but for small children and restless patients, the plaster of Paris bandage was necessary. In applying the pasteboard splints, the upper limb was bent at a right angle at the elbow, and the leg was straightened at the knee; the splints were well wadded, and fastened by bandages. The apparatus was generally applied immediately on the admission of the patients, and was allowed to remain, not only until all pain and swelling had left the joint, but until the constitutional symptoms had disappeared, and especially until the temperature had returned to the normal standard. The results of this treatment were extremely favourable as regarded the pain, the fever, and the duration of the disease. Even when the pain had been most severe, the fixing of the part was followed in a few minutes by so great an amount of relief, that the patients were almost free from pain. Of forty-five cases collated by Dr. Oehme, this result occurred in all without exception. After a time, varying generally from one to two days, all pain finally disappeared in the joints which were fixed. The period of final cessation of pain is later in those joints—the hip and shoulder—which cannot be so securely

fixed as others. To show the effect on the duration of disease, Dr. Oehme gives tables, comparing forty-five cases treated by fixing the joints with forty-five similar ones treated otherwise. While in the latter the average duration of the pain was 21·75 days, in the former it was only 13·11 days. The treatment by fixing the joints had also a favourable influence on the duration of the fever, which was distinctly shortened. It seemed also as if the fixation of a joint acted as a prophylactic in preventing the other joints of the same limb from being affected. From the observations now made, it may with safety, Dr. Oehme thinks, be asserted that the treatment of acute rheumatism with the immovable bandage reduces the pain to a minimum, shortens the duration of the fever, and essentially cuts short the course of the whole disease.—*British Medical Journal*, April 25, 1874, p. 550.

DISEASES OF THE NERVOUS SYSTEM.

10.—ON THE TREATMENT OF SCIATICA.

By Dr. FRANCIS E. ANSTIE, Physician to the Westminster Hospital.

Except in a very small and well-defined class of cases, there is no pretence for speaking of sciatica as a rheumatic disease.

One constitutional disease, however, not only is capable of producing sciatica, but probably does so with unsuspected frequency—viz., constitutional syphilis. It had always puzzled me to read of the marvellous successes which some observers had obtained with iodide of potassium in sciatica (seeing that in the ordinary forms of the disease I never found it useful), until I came across cases in my own practice where syphilis was evidently the cause, and in which I obtained immediate success by the use of that drug. I would advise you, in every case of sciatica that does not quickly yield to the kind of treatment which I shall presently recommend, to inquire very closely into the history of the patient as regards the possibility of his having had syphilis: and to show you the necessity of not being too readily diverted from the quest by apparent improbabilities, I may mention that one of the most marked successes I have obtained was in the person of a gentleman whose peculiar position seemed to put syphilis entirely out of the question. Here the primary syphilis dated as many as thirty years back, but a clear history of subsequent, though not severe, constitutional symptoms was obtained, and the treatment by iodide of potassium in large doses at once effected a brilliant success.

In the vast majority of cases of sciatica, however, you will find no trustworthy evidence of any diathetic condition resembling either gout, rheumatism, or syphilis; and you will be driven to the conclusion that the disease is a neuralgia pure and simple. Now let us ask ourselves what are the principles which should govern our prognosis and treatment of neuralgia in general and of sciatica in particular.

In regard to neuralgia in general, we are in the unfortunate position of lacking any positive information as to the precise seat and nature of the pathological change which is at the root of the malady. I say "positive" information, because post-mortem examinations of persons who, at the time of death, were suffering neuralgia, have been so rare that practically, from this side of the matter, we know but little. But we know enough to be quite certain that the apparent site of the pain need not be, and indeed scarcely ever is, the seat of the mischief which causes it. It is demonstrable, from accumulated clinical facts, that pathological causes which irritate the nerve high up in its trunk produce pain at the peripheric distribution; and it is also known that sensations which are excited by irritation of the central origin or nucleus of a sensory nerve, are, by a regular physiological law, referred to the periphery. As regards sciatica, we happen to possess precise knowledge of the immediately *exciting* cause in a considerable number of cases; for there is a rather large number of recorded instances in which irritative pressure of a loaded bowel, a gravid uterus, or a tumour, upon the nerve within the pelvis, has set up pains, distributed in the peripheral manner already described, and disappearing instantaneously on the removal of the cause of irritation to the trunk. But in regard to all neuralgias, a strong argument for the belief that the cause operates centrally rather than peripherally, arises from the fact that the pain simultaneously affects individual twigs of nerve which are widely separated from each other. In a facial tic, *e.g.*, it is not at all uncommon to find that the pain centres in two points—the supra-orbital (above the notch of that name) and the auriculo-temporal (in front of the ear); the former representing the frontal branch of the first or ophthalmic division of the trigeminus, and the latter representing the auriculo-temporal branch of the third division. Now, it might be said that the operation of some peripheral cause, such as cold, had excited pain in both these points of nerve; but clinical experience entirely forbids us to explain the matter thus, because the independent affection of widely separated twigs of the same nerve is quite as frequently observed where it is perfectly obvious that no peripheral irritation has been at work. If, then, the pain in two widely distant twigs of the

same nerve were not caused by irritation acting at the apparent peripheral site of the pain, we may next inquire whether the mischief is in the trunk of the nerve; and in a considerable number of cases of neuralgia we shall find that a portion, at least, of the mischief was caused in this way. Irritative pressure upon the trunk of a nerve has frequently, at any rate, so much share in the production of a (peripherally-felt) neuralgia, that the latter disappears immediately on its removal; thus a syphilitic periostitis may cause irritative pressure on a nerve within a bony canal, and anti-syphilitic treatment may immediately remove the pain. But we are bound to ask ourselves whether the irritation of the nerve-trunk is the whole matter, and for this reason—that various nerves, and especially the sciatic, from their anatomical position, are extremely frequently pressed upon by viscera, or by tumours of various kinds, and that if this cause were alone sufficient to produce neuralgia, that disease ought to be a hundred times more frequent than it is. It is not enough to prove, as we readily can, that the immediate cause of some cases of sciatica was the pressure of a loaded bowel, for, considering the very great frequency with which people carelessly allow their bowels to remain chronically constipated, sciatica ought to be one of the commonest diseases in existence, if the mere pressure were in itself enough to set up the disease. But sciatica is far from being a common malady, and we are therefore compelled to suppose that a predisposition to neuralgia existed in the nerve.

Now, in favour of the idea, that some *predisposition* is usually, if not always, present in neuralgic patients, and that peripheral irritation only serves as the final factor which elicits it, there is a very large mass of striking facts. Of these the most important are those of heredity: nothing is more certain than that neuralgic patients nearly always descend from families in whom the disposition to nervous diseases of various kinds is markedly developed. For detailed proof of this I must refer you to my work on “Neuralgia and its Counterfeits” (Macmillan), merely observing that since the publication of that book I have received a very large amount of corroborative evidence of the position now laid down. It has, indeed, been very confidently ascribed by a recent writer, that, whatever may be the case with neuralgias in general, no proof can be made out of the hereditary character of sciatica; to which statement I can only reply that the very considerable number of fully noted cases of sciatica which I have collected from my own observation, entitles me to say that sciatica holds a similar position to that of facial or other neuralgias as regards the question of direct and indirect inheritance. It is an off-

shoot of that neurotic constitution, the inheritance and the alternate transformations of which, in successive generations and different individuals, is so marked a feature in the history of many families. I do not wish now to occupy your attention too long with theoretical considerations, but in the work to which I have referred you you will find that I venture to believe is very strong evidence in favour of the opinion that the mysterious neurotic inheritance consists of an inherited tendency towards imperfect building of the central nervous system—that last and highest expression of the increasing tendency to differentiation of organs which corresponds with the upward scale of development in the animal series. I believe that sciatica follows the same rule which pertains to other neuralgias, and that underlying the fact of the external provocation which may have immediately induced the attack, there was a predisposition, the origin of which is to be traced to a feeble and mobile constitution of the centres in the cord from which the sciatic nerve takes its rise.

We will assume, then, that sciatica, in nearly if not quite all cases, requires a predisposition of that kind which descends in families that have marked tendencies to nervous disease. What, if any, are the particular circumstances which specially distinguish sciatica as regards the tendency to its immediate provocation, and the circumstances which are likely to make it a transitory or a lingering affection?

In the first place, the sciatic nerve is, beyond all others, obnoxious to the evil influence of muscular movement. Its large size, and its position among the muscles of locomotion, expose it to continual drags and pulls, and if once neuralgic pain has been set up in it the slightest amount of walking is sure to aggravate it and keep it up. And then, unfortunately, the exposed position of a large part of the nerve, from the sciatic notch downwards, makes the sitting posture equally bad with locomotion. In fact, there is no position, except lying prone upon a couch, which does not more or less worry the nerve. It is probably to their less energetic habits of locomotion that women owe their remarkable immunity, as compared with men, from sciatica. The most thoroughly obstinate and intractable case that I have seen in a woman was one in which the patient would not, for a long time after the commencement of her illness, take the physical rest which was imperatively required, but continued to limp about for hours daily. By the time this had been going on for some three months the case was thoroughly hopeless as regarded cure.

No disease is more markedly influenced by age than sciatica. It very seldom occurs before the age of twenty, and the zenith of its frequency is between the ages of forty and fifty. There

are, indeed, two quite different types of sciatica, putting aside the gouty, the rheumatic, and the syphilitic varieties (which have no constant relation to age). The sciatica of youth may be a severe complaint, and if injudiciously treated may become intractable and even wholly incurable; but, as a rule, it distinctly tends to spontaneous termination, except in so far as it is kept up by some vicious habit of body or mind. The effect of imprudent efforts to "walk it off" may convert what would have been a very trivial and temporary attack into a very obstinate one. Sexual excesses, especially masturbation, have an unquestionable tendency to provoke and to keep up sciatica in comparatively young persons, and alcoholic intemperance is a possible influence in the same direction. But it is essentially the neuralgias of later life—those which come on from the age of forty to fifty or later—which prove intractable and often incurable. The great rule, which holds true of all neuralgias, that the commencement of the process of bodily decay inaugurates a period in which the cure of these maladies becomes increasingly more difficult, applies in full force to the case of sciatica; and more especially is this effect observed where the degeneration of arteries forms a prominent and early feature in the tissue-degradations of the period of vital decline.

Brief and imperfect as this account of the pathology and causation of sciatica has been, it will suffice to indicate at any rate a more rational scheme of treatment than those which are adopted by routine practitioners. If we are to take first the varieties of the disease in which there is the most decided indication for treatment, we shall certainly begin with the syphilitic; and here I wish to repeat the caution already given as to not accepting too readily the idea that syphilis is out of the question. You will be most tempted to make this mistake when your patient is a lady of good character. But remember that she may have been infected by her husband, and that this may have happened (in conception) without the occurrence of any primary sores. Inquire carefully in such cases for any history of eruptions or sore throats, but especially ascertain whether there have been any abortions or still-births. Where the patient confesses that there has been chancre, you must not give up the syphilitic hypothesis simply because a number of years have elapsed with few or no recognisable symptoms of constitutional infection; this is a point which has been copiously illustrated in the valuable researches on syphilitic nervous diseases generally which have been going on during the last twenty years. The line of treatment is quite simple. You administer iodide of potassium in rapidly increasing doses till you reach as much as from sixty to 120 grains of the drug, or even much more, in each twenty-four hours. This very

rarely fails to produce a rapid and complete cure; but if it should prove ineffectual you may resort to the bichloride of mercury, sixty to eighty minims of the liquor ($\frac{1}{16}$ to $\frac{1}{12}$ grain) thrice a day. Very often it will be advisable to give cod-liver oil at the same time.

In the few cases of clearly rheumatic origin, also, we get a clear indication for treatment: the use of iodide of potassium with bark will usually be found to remove the inflammatory enlargement of the nerve, and give speedy relief to the pain. The prolonged use of Kreuznach or Woodhall Spa water is desirable, in order to render the cure complete and permanent.

In the cases where we have reason to believe that the conjunction of the gouty with the neurotic temperament is exercising a pernicious influence, the chief practical deduction must be that the patient should very sedulously avoid beer and all saccharine wines, and should be very moderate in his total allowance of food, especially of meat and other distinctively nitrogenous foods. The careful and prolonged use of Vichy or Neuenahr water may do great good.

But, after all, the gouty, rheumatic, or syphilitic sciaticas form but a small proportion of the mass of cases which may be encountered in practice. The important question, in dealing with ordinary sciatica, is—What am I to do with a disease which is essentially a neuralgia, but which is influenced by the special circumstances connected with the anatomical position and the functions which belong to the sciatic nerve?

In dealing with sciatica as a neuralgia pure and simple, we are fortunately provided with means which will give such immediate relief as will greatly solace the patient, and inspire him with that faith in his ultimate recovery which is always so valuable to the sick, and especially to the nervously sick. I have already explained how necessary absolute rest of the part is, and you will commence your treatment by arranging a proper couch on which the patient is to lie all day, and by making him understand that he is not merely never to put his foot to the ground (except for absolutely necessary purposes), but that he should always lie either prone on his face or (for a few minutes' change) on the opposite side to that affected. If he be in pain at the moment of your first visit, I advise you to give him a hypodermic injection of one-sixth of a grain of acetate of morphia on the spot. All this is only preliminary; it gives you time to look about you, and deliberately select your line of treatment.

In dealing with simple neuralgias there are four possible main classes of remedies—(1) constitutional, which include the regulation of diet and the employment of such medicines as are, in fact, supplementary aliments; (2) the removal of obvious

sources of possible irritation; (3) the narcotic-stimulant medicines; (4) local applications.

As we are not dealing now with gouty sciatica, what I have to say concerning alimentary treatment is mainly in the direction of insisting on a very nourishing diet, and especially the use of fats, beginning with cream, and going on to cod-liver oil. To this we may add the use of iron or arsenic, or both, in anæmic cases.

Dr. Lawson has correctly pointed out that sciatica is sometimes connected with an acid dyspepsia and a tendency to pyrosis. I believe that these cases are less common than he supposes, and that they are mostly found in those who happen to be the subjects of gout as well as of sciatica. At any rate, wherever such symptoms are found they should at once be met by the administration of effervescing alkalies, with small doses of quinine—say a grain of quinine in four ounces of Vichy or of Apollinaris water three times a day. The quinine is here given simply as a restorer of the digestive tone, not with any idea of producing a specific effect upon the neuralgia.

The only cases of sciatica in which quinine is likely to produce specific effects are those in which malaria is the exciting cause, and these are (in England) so rare that I have for practical purposes disregarded them. It is enough to say, here, that when we do encounter such cases we must treat them with the same full doses of quinine, administered before an expected paroxysm, as we should employ in ague itself.

2. The removal of obvious sources of possible irritation refers chiefly to two things. *Cold* should be guarded against by making the patient wear (night and day) a pair of thick flannel drawers. Intestinal irritation should be guarded against by thoroughly evacuating the intestines; it is best to do this by means of a good stimulant enema (ol. ricini ʒss., ol. terebinth. ʒss., gruel Ojss.) thrown high up.

3. Of the narcotic stimulant remedies, morphia, hypodermically injected, is much the most frequently useful, though it is scarcely that panacea for the disease which some have represented it to be. When I tell you that it can rarely be judiciously omitted in the early treatment of sciatica, I am very anxious that you should receive that statement in a reasonable way. The supreme utility of hypodermic morphia is due to the certainty with which (in moderate dose) it will cut short the pain without inducing narcotic depression. Pain is a complex and mysterious phenomenon, and among the many interesting facts concerning it is this—that the long continuance of pulsations, so to speak, of more or less rhythmical agony has a peculiar shattering effect upon the nerve, which leaves it *far more liable to pain than before*. Therefore you will do wisely to prevent, at any

necessary cost, the patient from ever having more than a few minutes of acute pain at a time. This can usually be accomplished by immediately using the hypodermic syringe when the attack commences; and at this period of the illness you may even give one-sixth or one-quarter of a grain twice in each twenty-four hours if necessary. But you are on no account to look upon hypodermic morphia as other than a temporary expedient to gain time for the recuperative powers of the system, aided by appropriate tonics, to conquer the morbid tendency.

4. Of local applications for sciatica (or any other neuralgia) some are used with one intention, some with another. (*a.*) There is a class of local remedies the sole action of which is to shield the terminals of the nerve-twigs from irritation by paralyzing their sensibility; the result being that the nerve and nerve-centre enjoy comparative repose while the influence endures. Veratrine ointment is one example; aconite liniment another. In using the former, you will do well to employ at first an ointment only half as strong as that of the Pharmacopœia (four grains to one ounce, not eight), or, if your patient has a delicate skin, you will produce inflammation and pustulation instead of simply numbing the nerves. Lin. aconiti, applied with a broad paint-brush, is more convenient and more certain, but very expensive where it has to be applied over a large surface.

(*b.*) Mild stimulation of the nerve is, however, on the whole by far the most satisfactory local method of treatment. This is done in two ways, either by blistering or by the use of the constant current. Blistering must always be used with precaution, and is almost wholly inapplicable to the irritable skins of *aged* patients. It is usually best to commence with the application of a blister, not directly to any of the painful points, but by the side of the spine at the junction of the lumbar and sacral portions. When the epidermis has been well distended with serum, the bladder is to be pricked with needles and drained of fluid without breaking the skin at all. If the malady prove at all obstinate, a series of these "flying" blisters placed successively near to (not actually upon) the foci of greatest pain will prove highly serviceable.

But in no instance of sciatica ought we to allow the pain to continue very long before putting in action a remedy which has approved itself in the hands of some of the best observers in Europe, of the highest value for sciatica,—I mean the constant battery current, a remedy so powerful (particularly in this form of neuralgia) that, but for the expense and trouble attending its use, it should be employed as the sole treatment in three-fourths of the cases of sciatica. It is absolutely

necessary to have a good instrument, such as Weiss's or Stöhrer's constant-current machines. From twenty-five to thirty-five cells will commonly be required, and the best method of application, on the whole, is the following:—The negative pole (the poles are broad moist sponges) is applied as nearly as possible opposite the roots of the nerves which form the sciatic, and the positive pole is applied in succession to the several foci of pain. The poles should be kept continuously applied for about three minutes at each of these situations, and this should be done either once or twice daily.

The prognosis of sciatica depends mainly upon the age of the patient, in the true physiological sense, and on the length of time during which the malady has already lasted. Eulenburg speaks of it as among the most curable of neuralgias, and so it doubtless is—in favourable circumstances and with the adoption of all proper remedies. But it may be rendered utterly intractable, either by the failing nutrition of the organism in the stage of bodily decline, or by the carelessness of the patient, or of the doctor, in not strangling the disease at an earlier period in younger subjects. No disease with which I am acquainted offers more opportunity for medical energy to find itself rewarded, or for medical supineness to incur not undeserved discredit.—*Med. Times and Gazette*, June 13, 1874, p. 637.

11.—THE EMPLOYMENT OF GELSEMINUM IN FACIAL NEURALGIA.

[Dr. J. SAWYER, of Birmingham, writes as follows:—]

I desire to draw attention to the value of gelseminum sempervirens in the treatment of some forms of odontalgia. Since reading Dr. Wickham Legg's paper, published in May last, advocating the employment of the drug in cases of odontalgia, I have frequently used the remedy for the relief of toothache and some allied affections among my outpatients. Gelseminum, commonly called the yellow jasmin, is not very generally known to English practitioners, although it has been largely used in medicine for some years in the United States. The drug seems to act mainly upon the nervous system, impairing the sensibility of the sensory nerves. American pharmacists prepare a liquid extract; the dose of the powdered root is from one to two grains; I have used a tincture, made from two ounces of coarsely powdered gelseminum root macerated in a pint of rectified spirit. In hospital outpatient practice, we meet with a large number of cases of neuralgic pains in the face and jaws, associated with carious teeth, but unconnected with any evident local inflammatory changes. The patients are frequently badly nourished women. In such cases, I have

given the tincture of gelseminum, in doses of fifteen minims every six hours, in an ounce of dill-water. Out of about twenty cases, I do not think the use of the remedy has failed to be followed by decided and lasting relief in more than three or four instances. The pain did not usually disappear till after the third or fourth dose. I have seen enough of the employment of gelseminum, to feel sure that more extended experience and careful investigation of its action will establish the drug as a valuable addition to our *materia medica*.

[Dr. E. MACKEY, of Birmingham, gives the following cases:—]

Case 1.—S. W., aged 30, confined eight months ago, not suckling, anæmic, for twelve months had been suffering with grinding, shooting pain in the teeth, jaws, and temples. The pain was generally worse from 9 to 10 a.m., sometimes at night. Eating brought it on. She had several stumps, but the dentist said that nothing was to be done. She had tried many medicines. She was ordered tincture of gelseminum, ten drops in water thrice daily. She was relieved by the first dose, and the improvement continued whilst she was under observation and taking the medicine—two to three weeks.

Case 2.—T.B., aged 39, complained of violent stabbing pain from the mastoid process, over the side and front of the head, coming on from 3 to 6 a.m. This had continued for three weeks. There was no known cause, except a fall on the part seven months ago. His general health was good. He was ordered five grains of quinine before the paroxysm, fifteen grains of ammonium chloride thrice daily, and liniment of chloroform. He got no definite relief, except from the last mentioned, and, at his next visit, was ordered three drops of tincture of aconite. From this he had much relief for a few days, when he relapsed. Pain was now felt more in the jaws and left side of the face. He was ordered ten drops of tincture of gelseminum, and got distinct and decided relief, which continued whilst under observation.

My note-book contains now many similar cases, which warrant me in adding my testimony to that of Dr. Legg, and to that of my friend and colleague Dr. Sawyer. I have ample evidence of the power of gelseminum to relieve pain, especially—I do not say only—in branches of the fifth nerve; and medicines that relieve pain are the most valuable we can have. In toothache from caries or irritated nerve, I do not know that we often think of any but local treatment, unless sometimes aconite or large doses of quinine; yet in very many cases, gelseminum will relieve markedly. Its use, as may be said of most neurotics, is not free from some danger; but I have only seen unpleasant symptoms once, and from an American liquid extract in doses of five to ten drops (the dose marked on

the label). These symptoms were dimness of vision and extreme prostration. They soon passed, but may serve to remind that the evidences of the physiological action of the drug are, loss of sight, double vision, headache, paralysis. Several cases of accidental poison have exhibited these; the few recorded fatal ones were all of children to whom extravagant doses had been given. The preparation I now use is a tincture made according to the usual pharmacopœial mode (*e.g.*, as tincture of aconite), but with two ounces of root to the pint of spirit. It was first prepared here by the hospital dispenser, Mr. Dewson, and is now kept by Messrs. Southall. The dose ordered was from five to twenty drops, and with this, among a large number of out-patients, I have had no cause for anxiety. As the "Therapeutic" corner of our Journal is now, and usefully, occupied concerning tetanus, I will point out that gelseminum, from its paralysing power, ought, *à priori*, to be of use in that malady.—*British Medical Journal*, May 2, 1874, p. 576.

12.—ON ELECTRICITY IN PAINFUL AFFECTIONS.

By Dr. G. VIVIAN POORE, Assistant-Physician to Charing Cross Hospital.

[Electricity would almost seem to be one of the most powerful anodynes which we possess, and its power in this respect is hardly as yet appreciated by the profession. In order to distinguish between real and imaginary effects, especially among the female hysterical class of patients, it is always well to begin with a mock application of it. If we could only in the same way test the actual efficacy of many other therapeutic agents which we employ, our knowledge of treatment and the effect of drugs would become more accurate than it is.]

Electricity will often cure pain, and much more often will alleviate it; and as an alleviator of pain it is not open, if properly used, to the objections which are applicable to most other anodyne therapeutic applications. By teaching a patient what are the anodyne effects of morphia, chloral, Indian hemp, or alcohol, we may destroy his sufferings only at the expense of his physical and moral well-being. Again, the after-effects of anodyne drugs are often only less disagreeable than the symptoms which they have removed. Electricity is not open to these objections, for, if it does no good, it does not, I believe, do any harm; and for this reason, if for no other, it deserves every trial as an anodyne. For a local pain, counter-irritation by means of a blister is often of great use; but there is this practical objection to the use of blisters, that if the pain return before the blistered surface has healed, we are debarred from

making a second similar application to the same spot. Electricity is a counter-irritant, and a very powerful and quickly acting counter-irritant; but it is something more than this—it has undoubted specific effects upon painful nerves; and it is not open to the objection of other counter-irritants, for it will not cause depression and exhaustion, and, if used with due care, may be applied again and again to the same region of the body.

Do not misunderstand these remarks, which are not meant as a crusade against time-honoured and invaluable remedies, but merely as an incentive to you, when you have (as in hospitals) the necessary apparatus at hand, to try the anodyne and counter-irritating effects of electricity.

All three forms of electricity are employed for the relief of pain, but the most generally useful for such purposes is undoubtedly galvanism.

You will find that some writers, when dealing with the anodyne effects of the galvanic current, lay great stress upon always placing the *anode* upon the painful spot. It is always pleasant to be in accordance with physiological facts, and it would increase our knowledge if we could be sure that the pain of neuralgia were always due to an exaltation of physiological properties which could be subdued by the production of anelectrotonus. Neither of these facts is certain, and you will find that Meyer—certainly an authority—says that the positive pole is to be applied near the centre, and the negative (the cathode) on the painful spot. For my own part, I have employed galvanism with much success for the relief of pain, but without hitherto paying any attention either to the direction of the current or the position of the poles. There is no relation, etymological or otherwise between *anode* and *anodyne*. There are many cases of neuralgia on record which have resisted every known method of treatment, but have yielded after a few applications of the constant current. In employing the current for this purpose, I think the true *locus morbi*, if it can be made out, should be included between the poles. If there is tenderness over any of the vertebræ corresponding with the part of the cord from which the painful nerves arise, one of the rheophores should be placed at or a little above this spot. The other rheophore should be applied *seriatim* to the various “painful spots” along the course of the nerve. In employing galvanism for neuralgia of the trigeminus, you must be careful—because of its effect upon the eyes—not to use too strong a current.

In some cases of neuralgia the pain is much aggravated by using the muscles which derive their nervous supply from the painful nerve. In these cases you will do well, when you

employ the current, to practise during its continuance a rhythmical exercise of the affected muscles. In this way the muscles, which have been thrown *hors de combat* by the pain, recover their tone, and it is only reasonable to suppose that the healthy exercise of the muscular and nervous function has a beneficial effect upon the healthy nutrition of the entire nerve, from its terminal twigs to its origin in the spinal cord.

The rational treatment of pain is, as with every other ailment, to remove the cause; and the complete and often immediate cure effected in many cases by electricity would seem to prove that the change produced in the nerves by the current is sufficient for this purpose. Many cases of neuralgia have been cured in this way at once. More than once I have completely removed a troublesome headache, not only of the transient type, but also of the migrainous periodic type, by a single application, for a few moments, of a weak current to the head. The pain under the left breast, so common in anæmic women, is often readily amenable to electrical treatment; and my friend Dr. John Williams, of University College, tells me that he has often found ovarian pain yield to the application of galvanism. When, however, the cause is something evidently beyond the reach of electricity we may nevertheless alleviate the pain for a time, often for many hours, by employing a galvanic current.

I remember the instance of a patient of this hospital who came to me suffering from "lumbago." That the "lumbago" was possibly caused by serious mischief in the cord or its coverings was evident by the great tenderness of the spinal column, the sensation of numbness in the feet, and occasional attacks of spasmodic pain shooting into the lower limbs. The patient was a poor basket-maker, and when first he came under my care he was scarcely able to move because of the pain. Counter-irritation and drugs having given him no relief, I determined one day to try the effects of the galvanic current. He was then unable to walk, and was obliged to perform the journey to and from the hospital in an omnibus. The galvanic current was applied to the back, the pain was completely alleviated, and he was able to walk away with little or no difficulty to the nearest railway station, half a mile off. This alleviation lasted about twenty-four hours, when his troubles returned. After this upon several occasions he presented himself for the purpose of having the current applied to his back, for its application was always followed by many hours of freedom from pain, while nothing else seemed capable of giving him relief. I then lost sight of him for six months, when he again presented himself in the out-patient room, with a large lumbar abscess and every sign of caries of the vertebræ, and he

was admitted to the hospital to be under the care of the surgeons. I mention this case merely as an illustration of the power of the current to alleviate pain, notwithstanding that such pain may be caused by the gravest, most permanent, and incurable organic changes.

Another instance no less remarkable was in the case of a middle-aged lady, who was sent to me by my friend Dr. Anstie. For more than a year she had been a martyr to occipito-cerebral neuralgia of the left side, of a most excruciating character. An examination of the left side of the occiput and neck revealed slight swelling, and, on deep palpation a crackling sensation could be felt in the region of the transverse processes of the first two or three vertebræ. Whether this was due to rheumatic inflammation or caries one could not determine, but doubtless this mischief, whatever it was, was the cause of her trouble, and the patient was accordingly informed that galvanism would probably do nothing for her. Her pains were much aggravated when the trapezius or the rotators of the head were moved. The current was employed about half-a-dozen times, the positive pole being placed at the occiput, and the negative moved in a labile fashion over the side of the neck and shoulder. The muscles were rhythmically exercised at the same time. Whenever the current was employed her pain was alleviated, the alleviation lasting for varying periods. On one occasion she was free from pain for nearly two days—a blessing which she had not enjoyed for months. Of course she was not cured, and she is probably now as bad as ever; but it surprised me not a little to find that, in the face of a grave permanent cause, we could give so much alleviation.

I have several times employed the current to alleviate the pains of intestinal colic, and often, though not always, with success, which in some cases was complete. I have, on the other hand, found many cases of pain in which the current apparently did no good whatever.

The galvanic current is, on the whole, the best for relieving pain, because it is itself the least painful form of electricity. Some writers—Meyer amongst them—strongly recommend faradisation, applied by means of a metallic brush or in the form of the “electric moxa” to the painful spot. Dr. Tibbits records a case of sciatica which was cured by franklinisation, and also a case of facial neuralgia cured in the same way. The method employed was to “charge” the patient, and then to “take sparks” along the course of the affected nerves.

There is a class of painful and sometimes semi-paralytic affections the pathology of which is shrouded beneath that most connotative word “rheumatic.” The ordinary “stiff

neck" is one of these. A man sits or sleeps in a draught with his neck in a strained position, and the next morning he cannot move his head without suffering agonies of pain. Again, a coachman, exposed to all weathers on his box, finds that the deltoid muscle of his left (the driving) arm, from exposure to wind and damp, and from being kept in one position, refuses its office; he is unable to raise his elbow, and the attempt to do so causes him much pain. The stiffness felt in the adductors of the thighs after a long ride in the wet, and the pain and stiffness about the region of the right shoulder which torment old sportsmen who have used the gun too much, belong to the same order of ailments.

Again, there are those various conditions which cause the symptoms of true "lumbago," situated at one time, I suppose, in the muscles of the back, and another time in the thick aponeuroses which cover them. Sciatica is another of the "rheumatic" ailments, the characteristics of which are that the pains do not follow the lines of nerve distribution very exactly, and that the pain is as much "muscular" as "nervous" in character, and is much aggravated by muscular movements.

Now in the vast majority of these cases you will find that faradism and galvanism, but especially the latter, act in a way which is little short of magical. If I were asked to state for what class of diseases electricity was of the greatest use, I should have little hesitation in saying the class which we are now discussing. I believe it to be essential that the painful muscles should be made to contract thoroughly. You may either do this by employing faradism, and so causing a strong artificial contraction; or you may use the constant current, and while the patient is under its anodyne effects you may make him thoroughly exercise all the muscles implicated.

Not long ago the coachman of an eminent London surgeon came to me complaining of the "rheumatism" of his left shoulder. On examining him, I found that his left deltoid muscle was almost useless to him. He could not, and had not, he said, for many years been able to raise his elbow, except in a very slight degree, and always with pain. He had exhausted the various liniments and oils, but with no benefit. I strongly faradised his deltoid muscle, and in a few minutes he was able to raise his elbow quickly and repeatedly above the level of his shoulder. You will come across many cases in which a cure may be effected quite as quickly and completely.

A very obstinate case of "lumbago" was lately put under my care in this hospital by my friend and colleague, Dr. Julius Pollock; and, as it shows very strongly what electricity will do in such cases, I will give you the facts.

W. G., a brass-finisher, aged thirty-five, was admitted as an

out-patient on the 16th of July, 1873. He was bent almost double, walked with the greatest difficulty, complained of intense "lumbago" pain, and gave the following history:—He had been accustomed to work a good deal with the lathe, standing up and moving the treadle with his right foot. About six years ago a pain came on gradually, affecting the back of the thighs and hips, which the medical man attending him called sciatica. The pain became so severe that he was obliged to give up work, and had not since been able to resume his employment. The pain left his legs, and finally settled in his back; and was most marked in the lower dorsal and lumbar regions, of a plunging, shooting character, aggravated by the slightest movement or the lightest touch, and implicating not only the dorsal muscles, but the intercostals as well. Movement had become so difficult that he was often as much as two or three hours in dressing and undressing himself. The back was considerably "bowed," the curvature being far more noticeable at night than in the morning. There was no indication whatever of any disease of the vertebræ, and the ailment seemed to be mainly muscular.

The positive pole was placed at the upper part of the spinal column, in the middle line, and the lower part of the back, and the lateral regions of the thorax were thoroughly sponged with the negative pole. The sponging had the immediate effect of annulling his pain, and produced great redness of the skin of the back and chest. While the current was being used he was made to exercise his muscles, to flex and extend his back, to rotate his spine, and to inspire and expire rhythmically and repeatedly. The result of this treatment was his rapid improvement, and he has been enabled to resume his employment, which he had discontinued for six years.

We have had before us at a previous lecture a patient who has been greatly benefited by galvanism. He was a middle-aged man, and when he applied for relief among the out-patients of this hospital he was suffering, and had suffered for nine months, from that most troublesome annoyance "tinnitus aurium." There was, he said, a noise "like the blowing-off of steam" constantly going on in his left ear. The least jarring of his body, even when walking, was most painful to him, and the roar of the London streets had become intolerable. I will not hazard any opinion as to the pathology of this disease; but it will, perhaps, suffice to say that a careful examination of the ear revealed to us nothing physically wrong with it, and that the clearing of the ears of wax, and the employment of nervine tonics and the nervine sedatives, had proved of no use. We then thought of giving galvanism a trial, and putting one rheophore in the patient's

hand, we applied the other to the meatus of the affected ear. He was immediately relieved, and at his next visit, a week later, he stated that he had been free from the noise for five days, and it then returned again, but not so severely. We repeated the application, and the result has been that he has almost entirely lost his trouble.

Electricity is no panacea, and often fails entirely to relieve pains; although I believe that, on the whole, the cases in which it is effectual would outnumber those in which it fails. Unfortunately, I am unable to give you any rules which should lead you to expect either failure or success in any particular case. As our knowledge increases our powers of prognosis will doubtless increase also.—*Lancet*, Aug. 29, 1874, p. 300.

13.—ON THE COMMON FORMS OF PARALYSIS FROM BRAIN DISEASE.

By Dr. H. CHARLTON BASTIAN, Physician to University College Hospital; and Senior Assistant-Physician to the National Hospital for the Paralysed and Epileptic.

In cases of this kind a person may be sitting at meals, walking, reading, or otherwise engaged, when—perhaps after a momentary giddiness, though without other sensation—he suddenly loses power over one side of the body. If standing, he drops down, owing to a giving way of the leg, and at the same time finds himself unable to move the corresponding upper extremity. When aware of the powerless state of his limbs, the patient may experience no pain, or other sensation than a feeling of numbness in this half of the body or in the limbs themselves. He is quite conscious, and may be able to speak more or less intelligibly, though at other times he is only able to communicate with his friends by means of signs. Now such a mode of onset is very frequent indeed, especially in the slighter cases of hemiplegia. Again, not unfrequently we are told by a patient that on awaking one morning he felt a sense of numbness on one side of his body, and was thus led to the discovery that the limbs of this side were paralysed. The injury to the brain takes place without producing any sensation sufficient to awake the patient, and this may occur even in cases where pain has been a marked symptom amongst the prodromata. Thus a woman aged forty-seven, who was under my care at the Hospital for the Paralysed and Epileptic, was seized rather suddenly with vertigo and pains in the back of the head. After half an hour she became sick, and the sickness frequently recurred, together with excruciating pains in the back of the head, for two days. On awaking the next

morning she found herself quite paralysed on the left side of the body and unable to speak.

Sometimes the onset of the hemiplegic symptoms is not absolutely sudden—the paralysis advances and spreads more slowly. In these cases the attack usually begins with a weakness in the hand or arm, though more rarely the loss of power is felt first in the leg. At other times the hemiplegia is ushered in by loss of speech and drawing of the face to one side. As examples of these latter modes of origin, I may cite the following brief details. One of my patients, a man sixty-four years of age, whilst walking, felt a tingling in the ends of his fingers, followed by weakness in the left arm, which increased, and afterwards extended to his leg and face: at the expiration of half an hour he was quite hemiplegic. Another patient, sixty-eight years of age, on getting out of bed felt uncomfortable, and went to bed again, when he was seized with a feeling of numbness in the left side, which was followed in about half an hour by complete paralysis of the corresponding arm and leg. Another patient, a woman aged fifty-two, whilst taking breakfast felt giddy and found herself weak in the right leg; and she gradually lost power in both leg and arm during the next twenty-four hours. The paralysis of these parts had then become complete, and, though there was no loss of speech, a thickness of articulation continued for some time. Again, a man, sixty-one years of age, awoke one night and found himself unable to speak. He felt no pain nor any unusual sensations, and in half an hour he regained his power of speaking. He went to his work the next day, but on awaking the following morning he found he had quite lost the use of his right arm and leg.

It happens, moreover, in certain rare cases, that a sensation of pain, either in the head or in one of the limbs about to be paralysed, has occurred as the initial symptom. Thus one of my patients, a man forty years of age, suffering from left hemiplegia, gave the following account of its mode of onset. He had suffered occasionally from sharp darting pains in the right side of his head, and also in his left arm, for about a month, when suddenly (whilst walking along a road) he felt an acute pain through the right side of his head, and thought he “heard a noise.” He would have fallen at the same time had he not caught hold of a support, owing to a sudden and complete loss of power in his left arm and leg. There was no loss of consciousness, but he spoke thickly for several hours. Another patient, a man sixty-seven years of age, after supper suddenly felt a stab-like pain in his right hand; he cried out, and fell down insensible, paralysed on his right side. I may state that in both these last-mentioned cases the sum-total of

symptoms seemed strongly to indicate that the pons Varolii was the seat of lesion. In another patient who was under my care a hemiplegic attack was ushered in by a still more special sensation, also tending to reveal or localise the seat of lesion. He was a man sixty-two years of age, who suddenly lost the use of his right side, without loss of consciousness, and without any sensation except a "stupefying smell," which lasted for two or three minutes.

Now, although I have described the three modes of onset of hemiplegia—the apoplectic, the epileptiform, and the simple—as though they were quite distinct from one another, you may easily imagine that these varieties become occasionally more or less merged and present during the same attack. Loss of power without loss of consciousness may exist for a very short time, and then the patient may either suddenly or gradually lapse into a condition of complete coma, owing to a sudden or gradual increase of the original lesion. Again, an epileptiform attack may supervene shortly after the onset of a hemiplegia which commenced either with or without loss of consciousness. Many varieties, indeed, in the mode of onset and subsequent progress are to be met with, which will, I hope, be more easily understood after what I have already said.

We must now turn to a consideration of the hemiplegic state itself, and as a starting point I shall take one of the most ordinary forms of hemiplegia. I shall describe the grouping of the symptoms occasioned by a lesion in and just outside the right corpus striatum, in order that this may serve as a type of the condition generally.

If a loss of consciousness has been occasioned by the shock to brain-function produced by the first onset of the disease, we must suppose our patient to have recovered from this condition after a few minutes or a few hours. Such temporary loss of consciousness need not necessarily exist, and in many instances is absent altogether. By careful examination we shall then be able to find that our patient presents the following peculiarities: (1) an absence of any decided mental disturbance; (2) slight thickness of speech; (3) more or less deviation of the tip of the tongue to the paralysed side when it is protruded; (4) partial and incomplete paralysis of the facial muscles on the side on which the paralysis of the limbs exists; (5) more or less complete loss of voluntary power over the left arm and leg; (6) a flaccid state of the muscles of these limbs, which respond naturally, or perhaps even a little too readily, both to the faradaic and voltaic currents; (7) some actual loss of sensibility as well as a feeling of "numbness" on the paralysed half of the body; (8) slight elevation of temperature on the paralysed as compared with the non-paralysed side of the body,

the difference being seldom more than one degree of the Fahrenheit scale.

Now the progress of such a patient in respect of these symptoms, where the lesion does not increase and the patient progresses favourably, is apt to be as follows:—

The *mental condition* continues undisturbed, or at most there is slight weakening of the general mental power of the individual, which may be accompanied by an increased readiness to the display of emotional manifestations—more especially to crying on slight occasions. This slight weakness may, however, entirely wear off after a time.

The *speech*, after a few days, or at most a week or two, loses its previous thickness. The slight difficulty in the articulation or utterance of words gradually passes off.

The *tongue* also soon again begins to be protruded with its tip pointing straight forwards, owing to the recovery of power in the genio-hyoglossus muscles on the paralysed side. Mistakes are apt to be made in reference to this point by a superficial observer, who does not allow for any marked deviation which may still exist about the mouth.

In a day or two, also, though perhaps not for a week or two, the *sensibility* of the paralysed limbs again returns to its normal condition. The subjective feeling of “numbness,” however, may persist for a longer period.

The *temperature* of the paralysed limbs soon sinks to the level of that of the opposite side, and after a time it is apt to become even a trifle lower. The paralysed limbs then are, and feel to the patient, colder than those of the opposite side.

The *paralysis of the face* begins to disappear sometimes after a few days, though more frequently after a few weeks. It may persist to some extent, however, for many months. Now, it is important that you should understand the kind of facial paralysis which is met with in these cases. It is, as I told you, both partial and incomplete—that is, it only affects, to an appreciable extent, some of the muscles supplied by the portio dura of the seventh nerve; and those which are affected are weakened rather than wholly paralysed. Those chiefly affected are the buccinator and other straight muscles going to the angle of the mouth and lip on the paralysed side of the body; in consequence of which the cheek on this side looks flat, the upper lip is less arched, and the angle of the mouth on the same side is decidedly lower than its fellow. As patients and their friends often say the face is “drawn,” though they refer especially to the opposite or non-paralysed side, where, owing to the unresisted action of the muscles on this side, the angle of the mouth is higher, especially when the patient laughs or speaks. Or you may judge of the extent of the

paralysis when, in answer to your request, the patient attempts to show his upper teeth. You may note that he can frown as usual, can lift his eyebrow and eyelid, and can close his eye on the paralysed side almost as well as on the other side of the face. It is only rarely that a trifling weakness of the orbicularis palpebrarum is apparent after the first day or two of the attack. And in judging of the existence or amount of facial deviation in any doubtful case you must not lose sight of the fact that an unsymmetrical condition of the mouth exists not unfrequently without any hemiplegic condition. This may be due to a comparative absence of teeth on one side, or it may have been a mere peculiarity long existing in the individual under observation. These may seem trifling matters, gentlemen, but they are oftentimes of great importance. You will hereafter have to form your opinion as to the state of patients who, in addition to slight symptoms of impending brain disease, seem actually to present some amount of paralysis on one side of the face. For such paralysis may exist alone for a time, as the first instalment or harbinger of a complete hemiplegia. You will see the importance, therefore, of recognising the kind and amount of facial paralysis which goes with hemiplegia, in order that you may be able at once to distinguish it from the mere local paralysis of the facial nerve due to comparatively external causes.

The last sign of the hemiplegic condition to disappear is the *motor paralysis of the limbs*. Recovery in this respect commences after a very variable period—weeks or months; and whilst in some few cases it may go on to a complete restoration of power, in by far the larger number recovery is almost arrested after the patient's condition has more or less improved. Now it is important to note that there is a certain order observed in the succession in which power is restored to the paralysed parts. The parts which are paralysed least are the first to show signs of improvement, and the parts which are paralysed most are the last to recover. But in all cases of hemiplegia in which the paralysis is not absolute, it is found that the arm suffers more than the leg; whilst of the several parts of each limb, the muscles about the proximal joints are least paralysed, while those for the movement of the distal joints are most powerless. So that, in accordance with what I have already told you, improvement as a rule begins to manifest itself by an increased ability to move the hip-joint, and is followed gradually by a recovery of power for the production of the more distant joint movements in the lower extremity, though at some variable stage of this recovery improvement also begins to manifest itself in the upper extremity—first as regards the movements about the shoulder-joint, and subse-

quently (if improvement should go so far) in those of the elbow, wrist, and fingers. The parts which are habitually employed in the most complex and purely voluntary movements are therefore the last to be restored to their normal condition.

In ordinary cases, such as we are now considering, the muscles remain throughout in a flaccid condition, and they undergo no special wasting—that is, no wasting beyond what may be accounted for by long-continued disuse or functional inactivity, conditions which cause them to assume a more soft and flabby feel than the corresponding muscles on the opposite side of the body. Again, the electric irritability of the muscles in cases of hemiplegia was long ago studied by Dr. Reynolds; and he showed that whilst during the first few days of the disease, or longer in some cases, the electric irritability, both for faradaic and voltaic currents, might be slightly increased, as a rule it was normal in the paralysed muscles of a hemiplegic patient. This fact many of you have now had frequent opportunities of verifying for yourselves, and it is one which often proves of much use to us in our endeavours to ascertain the cause of any given paralysis where its distribution is at all anomalous.—*Lancet*, June 27, 1874, p. 899.

14.—ON THE TREATMENT OF PARALYSIS BY ELECTRICITY.

By Dr. G. VIVIAN POORE, Assistant-Physician to Charing-Cross Hospital.

In treating paralysis the following rules may be of service—rules which are in part culled from Dr. Reynolds's invaluable treatise “On the Clinical Uses of Electricity.”

1. *If the paralysis to the will remain absolute, and if the contractility of the muscles be perfect, we do no good by persevering with electrical treatment.* This condition is often met with in hemiplegia. The patient is absolutely helpless on one side, although the muscles are in no degree wasted, and their irritability remains normal.

2. *If the paralysis to the will remain absolute, and if the irritability of the muscles be diminished, then electricity is useful, in so far as it helps to improve the nutrition of the muscle, and restore their normal degree of irritability.* The normal degree of irritability being restored (the paralysis to the will remaining absolute), then electrical treatment may be discontinued. The irritability of muscles may be diminished from mere want of use, and electricity may serve to restore this irritability.

3. We should bear in mind the advice of Sir Thomas Watson, that “our aim should be to preserve the muscular part of the

locomotive apparatus in a state of health and readiness, until peradventure, that portion of the brain from which volition proceeds having recovered its function, or the road by which its messages travel having been repaired, the influence of the will shall again reach and reanimate the palsied limbs.”

4. Whenever we meet with the *degenerative reactions* we should employ that form of current to which the muscles most readily respond, which is invariably the galvanic. We must persevere doggedly, and we may find our perseverance rewarded by the gradual return of the normal muscular irritability.

5. If the irritability to both forms of current has completely disappeared, we are not justified in persevering too long, nor in holding out delusive hopes to the patient. Nevertheless treatment should not be abandoned without a patient trial.

In treating paralysis with electricity, an exact diagnosis is, above all things, necessary, and we should be able to bring our knowledge to decide as to whether or not the paralysing lesion is of a permanent nature, or one that, according to our pathological knowledge, is or is not removable, or susceptible of improvement. If not, then our efforts to cure must of necessity be unavailing. It is in those cases where the paralysing lesion has disappeared, or has improved, and we have only to combat with the effects produced by it, that our efforts are the most successful.

Hysterical paralysis is especially susceptible to electrical treatment, and with it we occasionally work apparent miracles. When a patient who has no will to move her muscles sees that her muscles can be made to move in spite of her will, it is no wonder that such a discovery should exercise a wholesome moral influence over her mental condition.—*Lancet*, July 4, 1874, p. 4.

15.—CASES OF EPILEPSY AND HYSTERIA TREATED WITH FREE PHOSPHORUS.

By J. ASHBURTON THOMPSON, Esq., Surgeon-Accoucheur to the Royal Maternity Charity, London.

At the same time that the use of free phosphorus in medicine is beginning to make some way in England, and is now acknowledged to offer a *methodus medendi* capable of affording the most surprising good results, it must be confessed that the particular diseases in which the treatment may probably prove serviceable are very ill defined. It is, therefore, as a contribution to the mass of facts which it is necessary to accumulate before venturing to enumerate specific indications for its employment, that I bring forward this group of four cases.

The first is a case of infantile epilepsy, which has, as will be seen, some remarkable points about it.

Case 1.—A. B.; female; aged four years; father and mother healthy. I have attended three brothers of from one to three years of age, in fatal attacks of meningitis. In one case this was ascertained by post-mortem examination to be tubercular. The patient is a stout, red-faced, coarse-haired child, of the appearance which Londoners generally consider that of a "country child." To the educated eye she shows at once many evidences of peculiarities of nerve constitution. She talks fairly well. Was born with one tooth. Is considered to be a sharp child. In November, 1872, she had two long and severe epileptiform convulsions; in the second she presented the usual appearances. I could not detect any other evidence of disease; there had been no premonitory symptom; and she was very soon quite well again. On March 29th, 1873, she had one similar fit, which lasted fifteen minutes. As before, no warning was observed, and when the attack had passed over she seemed quite well. Twenty-five days after this she was observed to become very irritable; crying at the least provocation; picking quarrels with her brothers and sisters; sometimes sitting for ten minutes at a time, if undisturbed, staring at nothing, quite lost to what was going on around her, and flushing or turning pale at frequent intervals. On the twenty-sixth day, April 24th, she had another fit, lasting fifteen minutes. This time a little peevishness was observed to remain for a day or two afterwards. From that time to the present date, on every twenty-eighth day precisely, a return of these symptoms has been observed, with the variations hereinafter described. On May 22nd, the same sequence of events last detailed was observed. On June 17th, two days before the next fit was expected, she was brought for treatment. The premonitory flushings and absence of mind were well marked. She was, however, at the time of inspection, apparently in good health. There were no objective symptoms noticeable; but these generally disappeared under the excitement of seeing a stranger or any other small occasion. To take one thirty-sixth of a grain of phosphorus in the alcohol and glycerine mixture, every four hours. She should have had a fit on the 19th inst., but there was none; and indeed by that time she was quite well again. At the next period, July 17th, the treatment having been regularly persisted in in the mean time, there was again no fit; and the usual symptoms of nerve-disturbance lasted only two days, instead of five or six. On August 14th, September 11th, and October 9th, similar very slight symptoms were observed; the treatment had been intermitted on the 25th of July. On October 1st, she fell ill with

measles, and experienced a very severe but uncomplicated attack which left her very weak. The November period came round before she had recovered strength; and on the 6th of that month, without so much warning as usual, she became insensible, and was convulsed in a more violent manner than on any previous occasion. The insensibility persisted until the 11th inst., when she began to recover a little. During this time she continued in a state of clonic convulsion. The insensibility was complete. The temperature two degrees above the standard. The pulse ranged between 140 and 180, but it was at no time observed to be intermittent; and the pupils about the third day of the attack failed to answer to the stimulus of light. There were symptoms then of meningitis and of compression of the brain, and death was very confidently predicted. However, on the 12th she showed some signs of returning sensibility, and on the 13th she was quite recovered in that respect. Another week saw her as well as usual with the exception of a pain in the nape of the neck, which always attends the monthly manifestations. From that time to the present the usual symptoms of disturbance have recurred every twenty-eighth day, but in a degree distinctly greater than during and immediately after the time she was under treatment with phosphorus. She has not since been again placed under the influence of that drug.

In this case the remarkably regular return of the symptoms on every twenty-eighth day first attracts attention. The attack of March 29th may be regarded as the commencement of the series, although the next attack occurred on Thursday, April 24th—*i.e.*, two days short of twenty-eight. But this is the only exception to a manifestation of some neurosal disturbance on every fourth Thursday up to the present time. Since she had already had three consecutive monthly convulsions when placed under treatment, it is fair to attribute the intermission which then took place to the treatment commenced just before the fourth period. Notwithstanding the occurrence of the severe attack noted on Nov. 6th, on precisely the right day, whence it might be argued that it was of the usual epileptic kind, there was no doubt that it was in fact an attack of meningitis. Acting on this view, her treatment consisted in the hourly administration of tincture of aconite and bromide of potassium in doses of half a minim and one grain respectively. The excellent result which this method affords in hydrocephalus, and which Dr. Brunton has published in the Glasgow Medical Journal, led to its adoption in this case.

Closely allied to epilepsy is that other neurosal condition recognised as hysteria. The following observations made on patients in whom neuralgia and hysteria were concomitantly

present show such an improvement in the latter disease, while under treatment with phosphorus for the former, as appears to me to warrant a further inquiry into the power of this drug as a remedy for it.

Without venturing to attempt any definition of hysteria, it may be described as consisting in its first stage of a generally inefficient performance of the vital functions attended by general impairment of the nervous system; and very often a course of treatment not specially directed to the latter is sufficient to remedy both conditions. But at a later period, in confirmed cases, the nerve symptoms are often seen to persist in the most marked manner after it has become impossible to detect other evidence of impaired function. Under these circumstances, no indication exists which warrants the anticipation of success in treatment more from one drug than another. It is in these cases that moral influence is found pre-eminently useful; and probably cases are within the experience of every practitioner, in which recovery has been effected by this means alone, and sometimes instantaneously. The following instance illustrates the condition and the treatment referred to.

Case 2.—Some years ago I was requested to undertake the treatment of a young lady who was reported to have been bed-ridden and subject to fits for eighteen months. She had been under the care of various practitioners. On examination the patient was found to be a girl of eighteen years of age, well developed and stout, of good spirits and a cheerful manner; she was said to be of an equable temperament. Menstruation had commenced two years previously, and the periods had always returned with regularity; she had grown considerably since her confinement to bed; she ate and slept well; and all the other functions of the body appeared to be performed in a perfectly normal manner. But every afternoon at four o'clock precisely she had a fit, in which she appeared to be quite insensible; the body stretched out and stiffened. There was no clonic convulsion, she did not change colour, and the pulse and respiration remained unaltered, so that at first sight the case appeared to be a pretty clear one of malingering. But there was always present at these times a very great degree of anesthesia which appeared to affect the whole surface equally. The severe pain caused by pinching the skin violently between a pair of little hair tweezers called forth a languid acknowledgment of discomfort; but no amount of pricking with pins or pinching with the fingers or tickling elicited any sign of sensation at all. In ten minutes the fit would pass off entirely, and the patient would sit up and take her tea without at all interfering with her appetite for dinner two or three hours later. In addition the fingers and toes were always

immovably flexed and had not been known to suffer extension during a year past; and she affirmed that she could not stand on her legs. All kinds of treatment having been tried, and there being no special indications present, I determined to trust entirely to moral influence. But as a show of taking remedial measures materially lightens the heavy tax on the mental powers which this kind of treatment involves, I announced that her cure would be effected by galvanism. A trifling current from an ordinary rotatory machine was therefore passed down the spine for a few minutes on each of three days; and having by that time obtained the confidence of the patient, I informed her that she would have no more fits; and accordingly there were none. The galvanic machine was now thrown aside and she was persuaded that she could stand upright, and on the second trial a push in the back obliging her to take a step or two to the nearest article of furniture for support, showed her that she *could* walk. She was now soon persuaded that she could shuffle about the room with the assistance of a chair; but for more than a week she could not be brought to dispense with it, and it became necessary to take other measures. Not to prolong this illustration unnecessarily, it may be briefly stated that, declining to call in the aid of medicine, I resorted to the stick. The patient was set up in the middle of the floor conveniently contiguous to the chairs and tables, and started to walk with a push; and she was informed that whenever she sought assistance from the furniture, the hand stretched out would become acquainted with the stick. Of course there were tears and entreaties; but it was surprising to see her rapid progress when a few bruises made it understood that there was every intention of pursuing this line of treatment seriously; and by the time the hands were sore from repeated blows the patient could walk out in the garden like other people. In short, on the twenty-second day of treatment she walked through the streets to my house without assistance and full of thanks. It would seem in such a case as this that a habit of mind, to which general but actual disease had contributed in the first place, may be continued long after that disease has passed away, and that when all medicinal treatment has failed, it is still possible to effect cure by measures which can be supposed to act but in one manner—viz., by stimulating the nervous system and by thus rendering the mind capable of active and original exertion—capable of diverting itself from the introspective groove in which it has been confirmed by habit.

But it is not to be forgotten that to cause hysteria, as is the case in other diseases, the comparatively trivial causes in which it originates must occur in a subject predisposed to that dis-

order; and this fact points to some peculiarity in the nerve condition of such persons. The circumstances indicative of this predisposition appear to be an unduly irritable state of the nervous system, a want of just appreciation of the relations which should exist between externals and internals; a want, in a word, of self-control, which may at any moment appear as a true hysteric paroxysm, and which, if allowed to persist, terminates in a veritable paralysis of the will. But this condition is very frequently preceded and accompanied by symptoms which point to still more definite nerve derangements; by local or general anæsthesia or hyperæsthesia, by pains varying in situation and intensity, and which are perhaps never entirely imaginary, by sick headaches and neuralgia. The hysteric state would seem then to be one of *depression of nerve power*; and when this state of depression, which is an actual disease, has passed off, some of the symptoms to which it gave rise may be perpetuated by habit. In the latter case a moral stimulus may be found sufficient to recover the patient; but in the former—in the acuter stage of depression—this treatment alone is not permanently successful. Nor are there any known drugs which can be relied on to cure it, unless the one of which I am treating should prove to be an exception. Phosphorus may have other active properties, but it is nothing if not a stimulant, and it seems to exert that kind of power primarily over the nervous system. The association of hysteria with other neurosal symptoms, such as those last named, has come under my notice lately rather more frequently than usual; and these patients (who were being treated with phosphorus for neuralgia, and the various obscure pains which they generally complain of, and which I have been in the habit of regarding as neuralgic) have therefore been closely watched. I relate two of a series of six or seven cases in which a remarkable improvement in the general health has attended the same treatment employed with success to remove the special symptoms.

Case 2.—C. D. is a fairly well-developed girl of sixteen years of age. The menses appeared first about a year and a half ago, and the function has since been performed with regularity. She is somewhat pallid; and while she is at all times very excitable, for the last two or three months she has had a violent hysteric paroxysm once or twice a week. Her general health being pretty good, it was considered that these symptoms were due to debility consequent upon rapid growth, and advice was not sought until an attack of neuralgia supervened. It did not appear from the general information received that the pain was very severe, although the patient made a great deal of it; and it had lasted at the time of treatment about ten days with only slight and imperfect intermissions. It

affected the temporal and supra-orbital nerve branches on the left side of the head. One-twelfth of a grain of phosphorus in alcohol and glycerine was prescribed to be taken three times daily. At the third or fourth day the neuralgia disappeared and did not return; but in addition a striking change became manifest in her general condition. She had an attack of hysterics of the usual description on the second day of treatment; but from that time these fits ceased to occur. Her spirits began to be more equable, and her emotions more under control; she was neither depressed at times nor boisterously merry at others; she could laugh in moderation. The treatment, on these signs being manifested, was continued for a fortnight; and ten weeks afterwards her mother stated that there had been no return of the old symptoms. Three months after this, or about six months from the commencement of treatment, she again complained of the hysteric feelings; the same remedy as before was given, and the result was as satisfactory.

Case 3.—E. F. is a well-grown fresh-coloured girl of twenty, but it is obvious at a glance that she is hysterical. She had that fulness of the upper eyelid, and that entreating look about the eye which serve to diagnose hysteria without any further inquiry. Five months since her mother entered upon her fatal illness; it lasted for two months, and during that time the patient had a great deal of very arduous duty to perform, often passing two consecutive nights without putting off her clothes. Before she was released from this occupation she began to suffer from migraine. The pain always occupied the left temporal region exclusively, and would come on suddenly. For a few minutes previous vision would be disturbed, surrounding objects became indistinct, and while large things appeared to have green spots on them, print appeared to be surrounded with red lines; in a short time these sensations were followed by sickness, and the whole attack usually lasted some hours, only passing off after a little sleep. These attacks, which had persisted for three months, at the time of treatment, returned with tolerable regularity every other day; and, in addition, during the latter eight or ten weeks she had had a hysteric paroxysm at least once a week. She became listless and dull, being able to talk of little but her own health; and, having to take the position of mother to her younger brothers and sisters as far as might be, felt herself incapable of performing the duties entailed upon her. In addition to the above-named symptoms the menses returned every eighth day, and she suffered very much from flatulent dyspepsia, with irregularity of the bowels. A diet of lentil meal with milk and eggs, and abstinence from meat, with an alkaline mixture containing infusion of gentian and bismuth soon corrected the last-named

symptom; but the other trouble continued. She was therefore directed to take one-third of a grain of zinc-phosphide in the form of a pill and coated (Cox) every four hours.

Fifth Day.—A bad headache.

Thirteenth Day.—Has had no headache until this morning, when, having sat up very late last night, she had what she calls “half a headache.”

Fifteenth Day.—Was much upset yesterday by some domestic affair, and has quite a bad headache to-day. Menstruation.

Seventeenth Day.—Half a headache.

Twenty-fourth Day.—A bad headache. Menstruation.

Twenty-fifth Day.—With the exception of the seven days between the fifth and thirteenth days, she can scarcely be said to show much improvement in respect of the migraine, and she is very little better as regards the hysteria. The treatment with zinc-phosphide was therefore exchanged for three doses daily of one-twelfth of a grain of phosphorus dissolved in cod-liver oil.

Thirtieth Day.—There having been no headache, she was directed to intermit the medicine.

Thirty-first Day.—Slight pain occurring, the mixture was resumed, and continued for another ten days. There was no headache during this period, nor has any been experienced since—now two months. In addition, all the hysteric symptoms have disappeared, and she says that she is so well that she scarcely knows herself. It should be observed that as the headache failed to yield to the zinc-phosphide so the hysteric symptoms persisted unabated during its use; no sooner, however, was phosphorus exhibited in another form than her appearance at once altered. Her manner became cheerful; she no longer occupied the day searching out a new symptom to inform me of on the morrow: she began to interest herself again in her domestic duties, and she very shortly lost the peculiar physiognomy which I have alluded to above. In a word, in this case as in the previous, in curing the special nerve pain the general nerve condition was removed. In the last case it is not easy to estimate the importance of this happy result, for the patient came of a family in which consumption and insanity had occurred in many instances. The mother died of the former disease; the father maniacal.

I repeat, then, that having regard to the property of nerve-stimulation which phosphorus possesses, it is reasonable to anticipate that it may prove serviceable in cases of hysteria, which are indeed cases of nerve depression; and I believe that the cases which I have thus far had the opportunity of observing, and of which the two foregoing are examples, warrant this anticipation. To find a drug absolutely remedial of

hysteria is to prove in one way that that is a distinct disease; and since it is one in which, while it is widespread, and in many cases as serious as insanity itself, no medicine hitherto known can be relied on to exhibit remedial powers, the importance of such a discovery can scarcely be overrated, and if asserted with reason demands strict and instant inquiry into its value.—*Obstetrical Journal*, June 1874, p. 137.

16.—ON SOME NEW PREPARATIONS OF PHOSPHORUS,
WITH GENERAL REMARKS ON THEIR VALUE AS
THERAPEUTICAL AGENTS.

By Dr. C. H. F. ROUTH, Fellow of University College, London.

Some two years ago I had the pleasure of reading before the Medical Society of London a paper "On Overwork and Premature Mental Decay" (published by Baillière, Tindall, & Cox., London, 1873, pp. 32). The purport of that paper was to show that in many of these cases there was a chemical deficiency of nervous element, and that this last was imperfectly nourished, not receiving for assimilation the amount of phosphorus it required. This I proved pathologically by cases, and so far the paper was very well received, and gave an impetus to the use of phosphorus compounds in medicine. The British Pharmacopœia, by an Appendix just published, has recognised its therapeutical value for the first time.

My difficulty was to procure a safe phosphorus compound; but I think, now, with the assistance of my friend Mr. King, of Crawford Street, who is also a most intelligent practical chemist, we have succeeded in obtaining several, some of which I feel pretty sure will prove of unexceptional value as therapeutical agents.

In describing these *seriatim*, I shall specially refer to the action of the first of these, the *sol. phosphori medicati*, as exemplifying more clearly the action of phosphorus, and allude to the differences observed in their operation under the headings of the several preparations.

1. The first phosphorus compound which I tried was one which I called the *sol. phosphori medicati*. It is prepared according to the formula of Dr. Hammond, of New York. I have already described it elsewhere. Twelve grains of phosphorus are boiled in zi . of almond oil, and filtered, half of this is mixed with $1\frac{1}{2}$ oz. of gum arabic, adding 15 drops of wine aromatic. The dose is 15 drops, containing 1-24th of a grain of phosphorus, given three times a day with meals. As I have before remarked, this is not a preparation which every-

body can take. It sometimes operates very energetically as a poison, producing even in doses of ten drops intense sickness, temporary loss of power in the extremities, and a deadly pallor—in fact, it cannot be tolerated in these doses by some persons. It is more prudent to begin with five drops, gradually increasing the dose one drop daily, and in this way it can be brought up often to thirty or forty minims for a dose. I have found that in weak persons it is readily taken in five-drop doses with cod-liver oil, and in this manner readily assimilated. As much of the salutary effects of cod-liver oil is due to the phosphorus contained, it is probable this combination will in many cases add to its value. The effect of long-continued employment of this remedy is somewhat remarkable. It seems occasionally to produce an intense burning along the intestinal tract. Patients describe it as a coal of fire within them. This will, however, very soon disappear when the remedy is stopped; nor have I noticed any bad results to follow this symptom, which I look upon as evidence that the drug has produced its full effect, just as salivation may be said to denote that the mercury has produced its full effect. Short of this it produces in persons who are habitually chilly and weak an agreeable sensation of general warmth in the body, the comfort of which has been so enjoyable to some patients that they do not like to leave it off. Its effect on brain power and sexual impetus is, as I before observed, very satisfactory. I have seen in very many instances a man become very restless, nervous, irritable, wakeful, and with much loss of memory, gradually made quiet, more sleepy, and recover his powers of recollection. Here also I have noticed that several of my patients who, having become weakened from any cause, and who had once tried the remedy, have voluntarily gone back to it; the usual remark made, “I am always right when I take the phosphorus,” evidencing at least their faith in its virtues. It is not always prudent to give it in some varieties of spermatorrhœa. A larger experience of the drug has afforded me examples of cases of this disease in which it increased the spermatic flow, though not with the same tendency to cerebral pain and confusion which a night's loss in many of these examples produces. It becomes necessary therefore to combine its employment with the use of local astringent injections of tannin, zinc, or iron within the urethra. This effect, however, is seldom observed until the remedy has been employed for some time. Its first effect is always beneficial, and there is generally a marked relief to the nervous symptoms usually observed in spermatorrhœa.

Its effect upon some varieties of cutaneous eruption, eczema, acne especially is very remarkable from its rapidity.

The same remarks apply to cases of neuralgia, especially

of a chronic character, and in connection with uterine disease.

I may lastly remark that I believe the phosphorus compounds exert a beneficial effect in some cases of cancer. I speak here, however, more diffidently, for I require larger experience; but the pain has appeared to me to diminish, and the progress of the disease has been apparently retarded. I speak here entirely of phosphorus when administered by the mouth; of its effect locally in cancerous sores I am not in a position to speak yet as I would.

There can be no doubt, however, that this preparation acts uncertainly, and should be watched during the progress of its exhibition. After a time drowsiness is produced, and occasionally great dryness of the mouth, so as to lead to the suspicion that belladonna has been given. This last symptom is not of invariable recurrence, but when it does present itself it is very distressing. There is no doubt, again, that it clogs up the liver. Patients are apt to become very bilious, indeed, quite jaundiced, and in these cases the conjunctiva seems to be the first part affected even before the skin. A simple suspension of the drug for two or three days will cause all these symptoms to pass away, and then it may be again resumed with advantage; occasionally, of course, a brisk calomel purge is necessary.

Headache is occasionally a symptom observed, and if it persists after the second or third day, we may conclude the remedy will not be borne except after violent purging, or in very minute doses, if at all.

I have usually considered 1-30th of a grain of phosphorus quite a large dose enough to begin with, *i.e.*, about ten drops of this solution. But we should be prepared for sudden occurrence of the poisonous symptoms, sickness, vomiting, faintness, palpitation, and temporary loss of motor power. This solutio phosphori medicati is of all the preparations I have used that by which these poisonous symptoms may be most readily induced. I have been informed this is due to a chemical decomposition of the *vegetable* oil, which does not apply to *animal* oils. I therefore now recommend the use of sperm-oil, or neat's-foot oil, in lieu of the sweet almond oil.

2. The second is *phosphide of zinc*, also recommended by Dr. Hammond of New York. This salt is a black, hard powder, and tasteless; it is the only preparation of phosphorus which I have ventured to use in a solid state. It may be conveniently mixed with extracts.

It has been doubted, however, whether this compound is in reality what it professes to be, and not an insoluble compound which

cannot be appropriated by the economy; I may remark that it is so, first, because the chemical experiments made with it have been insufficient that this even could be doubted, and secondly, because the objectors have never tried the remedy therapeutically. If rubbed in a mortar it emits a distinct phosphorus (garlic) odour, and burns vividly in a flame, just as phosphorus itself would. Therapeutically it acts powerfully as a phosphorus compound, but not so powerfully certainly as the sol. phosph. med. I have generally begun with half a grain after meals three times daily, gradually increased to one grain, and even more. Apart from the good which it does in those cases for which phosphorus is useful, I have found it cannot be long borne in some cases of spermatorrhoea, as also increasing the quantity of the unnatural flow.

I have before alluded to the efficacy of arsenic in cases precisely analogous to those in which phosphorus is found useful. Exclusive of those cases, however, in which an ague element appears to enter, and in which arsenic is so effective a remedy, I have shown elsewhere, and have been here backed by the experience of others, that phosphorus sometimes succeeds where arsenic has failed.

3. It occurred to me, from seeing the beneficial effects of arsenic in many neurotic diseases, that it might be useful to obtain a compound of phosphorus and that metal. From the experiments made by Mr. King in this direction he concluded that the result was not to be obtained, the phosphide of arsenic proving quite insoluble.

However, after a very careful consideration it was decided to try the effect of bringing phosphorus and arsenic together in a finely-divided state in the presence of hydrochloric acid. This was found to be a difficult (indeed, a hazardous) operation, requiring great caution in manipulation. The resulting compound, however, proved very satisfactory. It has been called the *chloro-phosphide of arsenic*. It is free from offensive odour, capable of being largely diluted with pure water, and has then rather a pleasant taste. In its present solution it contains 1 grain of arsenic and 1.6 grain of phosphorus in the fluid ounce. The phosphorus and arsenic are so lightly held together that mere evaporation on hot coal is sufficient to disengage the former.

How, in the process of the formation of the chloro-phosphide, the hydrochloric acid acts is not clear; but in the same manner as the citrate of iron—an insoluble body—becomes the soluble ferri ammon. citratis on the addition of ammonia, so the insoluble phosphide of arsenic becomes soluble on the addition of hydrochloric acid, as the chloro-phosphide

of arsenic. The presence of iron in the arsenic, or lime in the phosphorus, is objectionable. The regulation of the temperature is also an important point in dealing with such a body as phosphorus. Disregard to this circumstance has led to batches of the preparation being spoiled, and to some danger ; but it has not yet been determined at which temperature the compound is best prepared.

As might have been expected from its composition, the chloro-phosphide is very effective in cases of neuralgia and marked ague cases. It also seems to act more as a general tonic than the other preparations, increasing the appetite ; and of the several preparations of phosphorus I have tried, it is the one which is the least likely to produce liver derangement. I have frequently noticed that patients who take the chloro-phosphide do not like to leave it off ; indeed, I fear, however I may dislike to say it, that this craving for phosphorus compounds, once they have been taken and found beneficial, may become a habit, in this respect resembling tobacco-smoking, or perhaps dram-drinking. The dose of the chloro-phosphide is from 15 to 20 minims three times a day after meals.

4. The *syrup of phosphorus* contains 1-60th of a grain in each fluid drachm, so that the dose should be about a teaspoonful. The only active ingredient in this syrup is phosphorus. It should be kept in a cool, dark room, and in small bottles for use. This compound resembles much a syrup which has been largely advertised. A weaker syrup than that prescribed in the Ph. Br. might be advantageous. The phosphorus, pure and finely divided, should be added with caution, and not beyond a certain limit.

This syrup is an exceedingly agreeably-tasted compound. This circumstance, in these tasty days, will make it very popular. In practice, however, it appears to act more powerfully as a phosphorus compound, and I have therefore, in the first place, limited the dose to begin with to half a teaspoonful, gradually increased.

It may be as well to remark, at a period when phosphorus is likely to be largely given, that turpentine, preceded by emetics, is the antidote. Dr Lichenstein has more lately called attention to this agent. The turpentine has been given in 12-drop doses in barley-water or milk. So far Lichenstein's experience confirms that of Personne, Andaus, and Kohlér. At the same time, it is right to remark that Dr. Schultzen, who had observed several cases of poisoning by phosphorus (30 or 40), states half have been got well, although no turpentine was given, and he attributes the recovery entirely to the vomiting induced.—*Medical Press and Circular*, June 3, 1874, p. 463.

17.—TUBERCULAR MENINGITIS IN CHILDREN.

By Dr. EUSTACE SMITH, Physician to H.M. the King of the Belgians, Physician to the East London Children's Hospital, &c.

The diagnosis of tubercular meningitis in the child is often very difficult in the early stage. The danger lies not so much in overlooking the disease when it is present as in suspecting its existence in cases where many of the symptoms are simulated by other affections. On account of the activity of the nervous system in young children, and the commotion into which it is thrown by the presence of acute illness, symptoms indicating functional disturbance of the brain may accompany many acute disorders. We should not therefore, because such symptoms are present, rashly assume that the disease must be a cerebral one, still less that it must be tubercular meningitis. M. Rilliet has given a wise caution against hurry in the diagnosis of this complaint. An opinion should be formed with deliberation, and be rested not upon one or two symptoms, but upon a broad view of the whole aspect of the case, otherwise we shall be very likely to fall into error.

There are numerous symptoms indicative of cerebral disturbance, which, although frequently present in cases of organic disease of the brain, are yet perfectly compatible with mere functional derangement: vertigo, headache, delirium, twitching and convulsive movements, irregularity of pulse, all belong to this class. One or all of these may be present in a case without necessarily denoting more than nervous irritability and distress which may be of purely functional origin. Tubercular meningitis is essentially an insidious disease, and there is a certain regular progression in the symptoms which should be always looked for. The condition of the child before the actual outbreak, then, becomes of especial importance, and a history of several weeks or months of languor and steady wasting greatly heightens the importance of the more definite symptoms of invasion. Vomiting, combined with constipation and an elevated temperature, coming on under such circumstances is always very suspicious. If afterwards the pulse becomes slow and irregular, and there is drowsiness, with occasional squinting or inequality of pupils, there can be little doubt remaining as to the nature of the complaint. In all such cases it is important to attend to the expression of the face. During the invasion stage the child looks really ill, and the features are pinched and drawn. In a case where tubercular meningitis is suspected, if this haggard expression be noted, however slight the other indications may be, we are justified in entertaining the most serious apprehensions.

A slow irregular pulse, especially if the intermission be complete, very greatly increases the value of the symptoms with which it may be associated. It must, however, be remembered that during convalescence from acute disease the pulse in a child often becomes slower and irregular, and may even completely intermit, especially during sleep. But in such cases the condition of the child is improving, while in the case of tubercular meningitis the slackening of the pulse is accompanied by increased mental heaviness, without any amelioration in the other symptoms.

In a typical case where all the symptoms of invasion are present, the diagnosis is not a difficult one, but very often some are absent. Thus, the disease may begin without vomiting, or this symptom may occur once and not be repeated. In other cases the usual constipation is absent, and the bowels may even be relaxed; but if there be headache and drowsiness with deep sighs, and if the pulse becomes slow and irregular, we have grounds for the very gravest suspicion. Irregular breathing is in such a case of the utmost importance, and, if markedly unequal, is perhaps more to be relied upon than any other *single* symptom in the diagnosis of tubercular meningitis during the early period. When in doubt at this period we should look for the slighter forms of nervous derangement, as these are more to be trusted than the severer, which occur, as a rule, later on in the disease, and are only occasionally present in the beginning. Thus, it is the exception for the disease to begin with a fit of convulsions or violent delirium, or other form of intense nervous excitement. In ordinary cases the symptoms are much less startling in their character, and must be looked for in order to be observed. Thus, the child may be noticed to contract his brows, wink his eyes, and avoid the light; his face wears a discontented or spiteful expression; the cheeks flush up and then grow pale, and the features are suddenly contracted at times with a strange grimace; the child puts his hand to the crown of his head, complains of headache, and sighs frequently; he is sullen and morose, has a peculiar fixedness of look, and answers questions dryly or not at all. Symptoms such as these are very valuable, because they, or at any rate some of them, are almost invariably present during the stage of invasion.

The diagnosis of tubercular meningitis has often to be made by exclusion. In all cases of acute illness beginning with nervous symptoms and with vomiting, the meningeal disease may be suspected, and we can only wait for further information before venturing upon an opinion. In the case of the exanthemata the appearance of the specific eruption enlightens

us upon the nature of the case. If the disease be pneumonia or pleurisy the characteristic signs of these complaints will be discovered on examination of the chest. Pneumonia, especially when it attacks the apex of the lung, is sometimes accompanied by nervous symptoms which bear a close resemblance to those of tubercular meningitis; a case of this kind has already been related (*Medical Times and Gazette*, 1873, vol. i., p. 437). Cases, however, constantly occur where the symptoms are anomalous, and where, however carefully the patient be examined, the most experienced physician must be at fault as to the nature of the disease. In such cases the medical attendant should proceed with caution, visiting the child frequently, and watching anxiously for any fresh and more definite signs which may point him to a conclusion.

There are two forms of disease in which mistakes are particularly likely to happen. Thus, exhaustion in children often manifests itself by vomiting and constipation, with drowsiness and great restlessness and distress. But here on inquiry we generally find a history of some recent acute attack, and the temperature is not elevated. Still it is often far from easy to give a decided opinion at once upon the nature of such an illness. Again, in that form of chronic abdominal derangement which I have elsewhere described under the name of "mucous disease," a sudden attack of gastric catarrh, with vomiting, constipation, and headache, may occasion very great anxiety, as the languor and wasting by which the disease had been accompanied may be easily mistaken for the premonitory stage of acute hydrocephalus. But in these cases, also, the temperature is generally low, and after a day or two, the gastric disorder having subsided, the child begins to improve. In gastric catarrh the heat of the body is not as a rule much elevated, often not at all, and the face does not show the pinched, haggard expression which is so common in intracranial disease.

During its later periods, tubercular meningitis may be confounded with any other cerebral lesion. To make a distinction, it is important to obtain a clear account of the history and progress of the illness, for the insidious beginning of tubercular meningitis is an important element in the diagnosis. In a practical paper it will be unnecessary to go through all the diseases which bear more or less resemblance to the one we are considering. For fuller information on this subject the reader is referred to Dr. Gee's admirable treatise in Dr. Reynolds's "System of Medicine." There are, however, three diseases which it is not so very uncommon to meet with in practice, and which may be briefly alluded to. These are—simple

meningitis (without grey granulations); cerebral tumour; and abscess of brain, due to disease of the petrous portion of the temporal bone, with suppuration in the mastoid cells.

Simple meningitis begins suddenly in a state of good health or after an accident, with either violent convulsions, high fever, and rapid breathing, or with severe headache, fever, and sickness; the patient then gets drowsy, and is excessively restless; the restlessness passes into wild delirium; and after a very few days collapse comes on, and the child dies exhausted. Constipation is slight, and the pulse, although intermittent, is not slow as a rule. The disease then begins violently, and increases in violence as it goes on. Its duration is short, and it rarely lasts longer than a week.

Cerebral tumour also manifests itself suddenly, generally by a convulsive fit, or some other severe nervous symptom, such as a paralytic lesion, with violent paroxysmal headache, and perhaps vomiting. It only exceptionally begins with the invasion symptoms common to tubercular meningitis—viz., vomiting, constipation, and headache alone. In any case the course of the two diseases is very different. In tumour the convulsions are violent, frequently repeated, and are followed by paralysis or rigidity in the convulsed parts. The paralytic lesions are various, and usually include some one of the organs of special sense. As a general rule, the intellect is preserved till late in the disease, and there is none of that drowsiness and dulness of mind which is one of the most prominent symptoms in tubercular meningitis. If, however, the tumour be tubercular, there are often granulations in the meninges causing meningitis. In such a case the symptoms of cerebral tumour would be confused and combined with those dependent upon the complication.

In cases of disease of the pars petrosa, with formation of intracranial abscess pushing up against the brain, the symptoms are much the same as those of cerebral tumour, provided pyæmia be not present. If there be pyæmia the case is complicated by the ordinary symptoms of that disease. The existence of long-continued otorrhœa with a history of previous deafness is a valuable assistance in the diagnosis of such cases.

Tubercular meningitis is almost inevitably fatal, and we should not therefore allow ourselves to be deceived, by any sudden amelioration in the symptoms, into a belief that the child is about to recover. Such sudden changes are not uncommon, but they are most deceptive, and when they occur it is our duty to warn the friends of the patient of the little confidence that can be placed in the apparent improvement.—*Medical Times and Gazette*, July 18, 1874, p. 71.

18.—THE PROGNOSIS AND TREATMENT OF CHOREA.

By Dr. FRANCIS E. ANSTIE, Physician to Westminster Hospital.

[It has been proved beyond dispute that chorea is occasionally the result of embolism of a cerebral vessel, and from the knowledge of this fact it is natural that we should be tempted to imagine embolism as actually present in all these very numerous cases. This would be a grave mistake. There is, however, a connection between rheumatism and chorea which is more widely applicable to the explanation of the facts than the theory of embolism.]

In the inquiries which I have carried on for many years respecting the pathology of neuralgia, one of the most pressing questions for solution appeared to be the kind and degree of connection which existed between neuralgia and the rheumatic diathesis. There is no need to detain you with the details of that inquiry; suffice it to say that I was compelled to the conclusion that rheumatism is comparatively rarely a direct cause of neuralgia: the truly rheumatic cases of that disease are a very limited group. At the same time, however, I began to perceive another kind of connection between rheumatism and neuralgia, which I believe will also be found to hold good between rheumatism and chorea. I observed that the hereditary character of rheumatism, which is sometimes well marked, is associated with hereditary tendencies to nervous diseases of various kinds.

[After relating the history of a family illustrative of this in a marked degree, Dr. Anstie observes,]

But this is what I particularly wish to mention:—So far from the chorea universally occurring in the victims of rheumatism, it was often strikingly the reverse.

The prognosis of chorea has assumed a great importance to me in consequence of what I have seen in hospital and private practice; and surely, it is a subject much too lightly treated in the ordinary systematic works on medicine. No doubt there are men who appreciate all the gravity of the subject, but they are in a minority.

I have observed a large amount of suffering and disaster to health of which chorea has either been the direct cause or at least the prophetic forerunner. I know of few things which would more incline me to think gravely of the future of a family than the fact that I had found it much invaded by chorea.

[Of the accidental causes of chorea]

The most regular in its operation is insufficient food. Where this has been the main cause of the chorea, or the chief reason

why the chorea is severe, we may hope everything from the effects of copious and generous nutrition.

It is certain that where we can *permanently* raise the scale of nutrition of a patient who has been brought into chorea chiefly by starvation, we may often save his nervous health, once for all.

The next, and one of the gravest questions in estimating prognosis of chorea, is whether the affection occurs in the pre-sexual period, or after puberty has commenced. No doubt every experienced practitioner is more or less aware of this fact, yet I think it is possible to show its magnitude and its importance more clearly than they are usually seen.

[Dr. Anstie then relates two cases in which recovery was due to the patients not having yet reached the perilous period of life which commences with puberty, and then continues :—]

In very gloomy contrast with these cases are others which I have been unfortunate enough to see in the course of my experience. One was a girl of 17, who had menstruated from the age of 13, always profusely. She came into Westminster Hospital, not looking half so ill as the little boy whom I have mentioned; but she had not slept for several days, and was in continual general choreic movement—head, arms, legs, features, were in perpetual action. Another twenty-four hours of this made a fearful change; she got into an almost maniacal condition, and died perfectly worn out in three days from admission and in about twelve days from the beginning of the illness.

In the next place let me say a few words on the influence which the facts of heredity ought to exert in shaping our prognosis of chorea. And in this respect there are two things to be considered—the prognosis as to the result of the individual attack, and the prognosis as to the patient's future life. In respect to this, there are certain facts not commonly known, as I suppose. If the family from which the patient comes be on the whole strongly disposed to insanity, the chorea itself will not unfrequently be a trivial affair, but it is likely enough to be the first intimation of a coming mental degradation.

It occasionally happens that a boy or girl, born of a family which has numbered many nervously diseased and a few really able members, has chorea in childhood, but in place of getting intellectual harm from it, he seems to date from the period when it leaves him a most marked increase in his intellectual powers. It by no means follows that his moral nature will improve *pari passu*; indeed the spectacle of a “bullocky Orton” turning into a clever rogue like the Claimant, after a youth beset with chorea and semi-imbecility, is, I believe, less uncommon than would be supposed by most persons.

[Now as to the treatment of chorea.]

One broad assertion which is frequently made is, that simple chorea always runs its own course in either four, six, or eight weeks, and then leaves spontaneously. No doubt it does so in very many cases, yet the longer one studies chorea the more one finds that there are many exceptions to this. Not to speak of the pretty frequent cases where chorea, interrupted for the moment by the onset of some acute disease, returns with double vigour and implants itself with double tenacity in the enfeebled organism of the convalescent patient, there is a far from inconsiderable number of simple cases of chorea which tend to drag on beyond that period of three months which, as Jaccoud justly observes, marks the limit at which chorea passes into the intractable type. I have become convinced that there are several means by which at least the disease can be kept to the shorter and more normal term. And besides this, I do not doubt that we can sometimes intervene in the terrible acute cases, with the effect of saving life and preventing the patient from becoming imbecile or epileptic.

In commencing the subject of treatment it is necessary to remark that if embolism be considered the probable cause of any given case of chorea, medicinal treatment must surely be vain. Tonics and cod-liver oil may possibly be of some use in improving nutrition, but we must necessarily wait for the removal of the disease by natural processes. When, therefore, a person who is notoriously suffering from valvular disease of the heart suddenly gets an attack of some kind, paralytic or epileptic, and thereupon passes into a state of chorea, there can be no sense in adopting any special plan of treatment beyond that already indicated.

In the very numerous cases, however, in which there is neither rheumatism nor heart-disease present, we should be very foolish, in my opinion, to give up the attempt to interfere with the disease; and indeed the great discomfort which the patient suffers, and the alarm which his friends experience, will not allow us to fold our hands. I wish therefore to mention the few things which I have found really useful; and in the first place we will take the gravely threatening acute cases, such as those of the two children in the Belgrave Hospital, who have been already referred to. The boy, aged 6, is a remarkable monument of what the organism will endure from physicians. We were determined to test the power of *succus conii* to the utmost, and, commencing with 3 vj daily, we got up to 8 ounces daily of the *succus* without producing any more effect than as if so much water had been given. I wish particularly to mention that the preparation was got from three

different and first-rate chemists in succession, so that there is no reason to believe that we were employing an inert specimen; indeed, I believe our house-physician proved in himself the physiological activity of the sample of which the patient took 24 ounces in three days. The same result happened with the girl who was under treatment at the same time; however, she never got beyond three ounces of the succus daily.

In the excessively severe case of the boy, we tried not only conium, but bromide of potassium, camphor, ol. morrhuæ, and zinc in large doses, all with only momentary effect. We then tried Jaccoud's plan, which I have found extraordinarily successful in several cases: viz., we sprayed the skin over the whole length of the spine with ether twice daily. I will not positively say that it was *propter hoc*, but I will say that immediately *post hoc* the symptoms greatly amended, and in the course of a fortnight the lad was perfectly free from movements. We kept him in the hospital several weeks longer, in order to re-educate his speech, which was grievously deficient. But by the time he left the house he could speak quite well and manage all his limbs; nothing amiss remained beyond a slight tendency to grimace. The girl, with whom succus conii, camphor, ol. morrhuæ, bromide of potassium, and large doses of zinc had entirely failed, began to improve immediately on taking liq. arsenicalis in 5-minim doses, afterwards reduced to 3 minims. I am convinced that in one of these cases death, and in the other a protracted and very serious illness, were avoided by the use of remedies; and I will just say here that arsenic as an internal remedy, and the ether spray applied to the spine, have given me solid results such as have been obtained by no other remedy. The ether spray stands somewhat intermediate, I suppose, between the ordinary shower-bath and the spinal ice-bags of which so much has been said. Cod-liver oil and iron, however, are very useful in anæmic and generally debilitated subjects. And there is a special class of cases connected with violent ovarian excitement, or complicated with epileptic tendencies, in which the bromide of potassium is invaluable, and is the one remedy.

In the terribly dangerous acute cases of young women, especially where there has been sexual excitement and exhaustion, I believe nothing does any good but free stimulation, regular feeding per rectum, and opium in large doses. I regret to have to express my complete distrust in chloral, and in a host of other remedies that have been proposed.

In the milder stages, and especially with a view to the future bodily and mental health, there are a variety of precautions which ought to be adopted.

Under these circumstances we often get the first considerable improvement by the use of the shower-bath. This old-fashioned and useful remedy is a two-edged weapon, and if employed in cases where the nervous system is too severely disordered and prostrated may produce bad results; but in the milder and more chronic type of chorea it is still worthy of being called a sheet-anchor. It gives that preliminary bracing to the nervous and moral tone, without which we may fail to get a leverage for other treatment. There is no absolute necessity for beginning with cold water, and in delicate subjects it is better at first to use it tepid; but we can soon advance to the cold shower-bath daily or twice daily. The second item of treatment is the training of the muscular system. We can do much more good with this, in most cases, than with medicine, provided that the patient's nutrition is kept thoroughly good. This lesson has been particularly enforced by my colleague Dr. Radcliffe, who always insisted much upon the value of muscular exercises which require rhythmical movements. He used, in Westminster Hospital, to give the choreic children skipping-rope exercise; and this will be found a very useful thing either in the absence of means for more elaborate training, or as introduction to more studied and complicated movements. I may conclude by pointing out the necessity for careful training in speech, where that faculty has been at all impaired, and also of attention to mental education in every case. The mere cessation of the choreic movements ought not to make us consider that we have done our work; it is most important that a judicious system of education should be at once adopted to strengthen the mind, and especially the memory. And although every care should be taken to avoid harsh or unkind treatment, it is very needful that a firm and regular discipline should be established, for anything like slackness or vacillation on the part of the educator is sure to be reflected and exaggerated in the behaviour of a nervously weak child.—*Practitioner*, June 1874, p. 431.

19.—ON THE DEEP INJECTION OF CHLOROFORM FOR THE RELIEF OF TIC DOULOUREUX.

By Dr. ROBERTS BARTHLOW, M.A., Professor of the Practice of Medicine in the Medical College of Ohio, U.S.

[Dr. Bartholow published in 1873 in the Cincinnati Clinic an account of three cases of tic douloureux in which the deep injection of chloroform had given marked alleviation of pain, and his subsequent experience justifies him in regarding it as a useful therapeutic expedient.]

By the "deep injection of chloroform" is meant the insertion of the needle of the hypodermic syringe deeply into the tissues, and the injection of the chloroform into the neighbourhood of the nerve trunk, the peripheral distribution of which is the seat of the pain. In the cases which have been reported, the infra-orbital branch of the nerve has been the seat of the *tic*. In these cases the operation consists in passing the needle under the upper lip in the direction of and near to the infra-orbital foramen, and then injecting from ten to twenty minims of pure chloroform. Very considerable pain is experienced at the moment of the injection, and for a few minutes subsequently, but this presently subsides and is succeeded by a feeling of numbness and of anæsthesia of the parts into which the chloroform diffuses. A puffy swelling quickly forms at the site of the injection, and an induration, which remains for several days, follows. The numbness of the lip and cheek continues for a variable period—for a week or more. Systemic sensations, such as giddiness and sopor, due to the diffusion of the chloroform into the blood, are soon experienced, but these effects may be scarcely perceptible, and are never alarming. Indeed, the results, so far as systemic effects are concerned, may be regarded as absolutely free from danger. So much swelling and induration occurring at the site of the injection, must occasion apprehension of the formation of an abscess. Although it would be rash to deny the probability of such an untoward occurrence, yet this accident has not thus far happened in any of the reported cases of the deep injection of chloroform.

In order to ascertain more satisfactorily than I could from an examination of the patients, the degree of suffering which attends the injection and the extent and duration of the resulting numbness, I practised an experiment on myself, by inserting fifteen minims of Squibb's chloroform under the skin of the calf of the leg. The pain was by no means so severe as I had anticipated, and indeed could easily be borne. Considerable swelling resulted, and an induration as large as a filbert continued for two weeks, when it disappeared without suppuration. Immediately after the injection numbness was experienced about the site of the injection, but it then extended downwards, and on the following day had reached to the bottom of the foot. A space in which the sense of touch and the appreciation of temperature and pain were markedly diminished, existed from the point at which the chloroform was inserted to the hollow of the foot, although somewhat irregular in shape, at least two inches in transverse diameter at any point. This numbness and insensibility continued for three months, and could then be ascertained to exist by directing the attention to the part.

It is obvious that chloroform injected into a part modifies the power of the nerves to conduct impressions to the sensorium. When it is injected into the deeper parts of the face, it comes into relation to vessels having an intimate connection with the intracranial circulation. As is, of course, perfectly well known, the facial vein communicates with the pterygoid plexus and the cavernous sinus. In addition to the local benumbing effect, chloroform injected for the relief of tic douloureux must quickly act on the centres of conscious impressions. As pain means—in the ordinary cases of neuralgia of the fifth, at least—an irritation of the nerve trunk, the perception by consciousness of this impression, and its reference outwardly to the peripheral distribution, we may assume that chloroform causes an interference or interruption at several points in the circuit. Not to occupy space further in these speculations, however, I give briefly some details of the cases treated by the injections of chloroform.

Case 1.—Man, aged 45, book-keeper by occupation. Without constitutional taint. He experienced occasional paroxysms of facial pain on the right side for six years, and had various dental operations performed, including the division of all the branches of the nerve distributed to the teeth in the right upper jaw. The paroxysms, however, increased in number and severity until, for the six months previously, they occurred every few minutes. An attempt to smile, to wash his face, or to eat, brought on horrible paroxysms of pain and convulsions of the muscles. A great many remedies had been used, including the galvanic current and the hypodermic injection of morphia, but none of these had afforded any but the most temporary relief. I injected twenty minims of chloroform in the manner above described. This operation brought on a frightful paroxysm of pain, which, however, soon subsided and was succeeded by complete relief. He experienced very considerable giddiness and drowsiness in a few minutes, and these sensations continued an unprecedented time, for he was not entirely free from them for two days. An induration which followed the puffy swelling continued at the site of the injection for a number of days, but finally disappeared without suppuration. He presented himself at the expiration of several weeks to say that he had been absolutely free from pain, and he had gained so much in flesh, and his countenance was so much improved in expression, that I scarcely recognised him.

For a number of months he continued free from pain or uneasiness of any kind, but he began after this to experience intimations that he was to be visited by his old enemy. About three months ago, these forebodings were realized in an actual paroxysm (since the publication of my paper). The attack

was not nearly so severe as his former ones, but lest he might experience them in their old intensity, he reported at once for treatment, and I injected five minims of chloroform. This injection gave him complete relief again, and when I last saw him, a few weeks ago, there had been no attacks, and he had had none of the warnings which he had learned to interpret as indicating the return of his pain.

Case 2.—This was a farmer aged 56, otherwise in good health. He had a history of facial pain in the right superior maxillary division of the fifth for twelve years. There had been a gradual diminution in the duration of the interval and a marked increase in the severity of the attacks. For the past year they had occurred a number of times each day. As in the case narrated above, the least motion of the face induced a paroxysm at any time, but the pain occurred spontaneously very frequently. As indicating the horrible suffering which he underwent, it will suffice to say that his family could not endure the sight of his violent sufferings and facial spasms when he attempted to eat; hence he was compelled to take his meals alone.

When I raised the lip for the purpose of inserting the chloroform, he experienced such a frightful paroxysm that I was compelled to desist, and it was only after repeated efforts that I succeeded in giving the injection. The result was fortunate. He had complete relief for some days. The pain then returning, he reported for further treatment, and another injection was administered. The relief continued much longer. In all, I believe four injections were practised, and he seems to have been permanently relieved, for months have passed without his needing further treatment.

Case 3.—This case occurred in a broken-down subject, with a history of syphilis. Intracranial lesions probably existed (exostosis?). He was a patient of my friend and colleague Prof. James T. Whittaker, M.D., who used the chloroform injection at my request, having exhausted the usual remedies, including galvanism and the hypodermic injection of morphia. The first injection relieved him for three days, when a very large dose of morphia, hypodermically, gave him but an hour or two of very partial relief. The pain returned, and the chloroform injections were repeated, with, however, less relief. A great deal of swelling of the face ensued, and the patient, probably disappointed in the result, ceased attending.

Case 4.—This case was reported from Georgia, and was similar in details to Cases 1 and 2. Unfortunately I cannot find in London the journal containing it, but I am able to state that the physician reporting it believed that permanent relief had been obtained.

Case 5.—This is an extremely important case reported by Dr. J. B. Mattison, of New Jersey, and to be found in the Medical Record of New York, May 1, 1874. I present here merely an abstract of the most necessary details.

C. D.—Trifacial neuralgia had existed for two years, when it suddenly disappeared and was succeeded by sciatica of left extremity which continued twelve years. The neuralgia then returned to its original site. “The paroxysms followed each other at intervals of four to six hours, at the beginning of the attacks” after its return to the fifth, and the duration of the intervals diminished “until they were occurring every few minutes. During the seizure the muscles of the affected side twitched convulsively, the face was distorted, and the patient, laying hold of anything within his reach, groaned aloud in the agony of his distress.” The patient had also acquired the morphia habit. A great many remedies were used for his relief without avail, and “morphia subcutaneously in increased amount, reaching at one time five grains at a dose, without any benefit whatever.” Dr. Mattison, proceeding in the manner I had indicated in my memoir, injected twenty minims of chloroform, with the result of producing with the greatest exactitude the effects which I had described, including “the *entire subsidence* of pain.”

“The relief from suffering continued complete for one hour and fifty-five minutes, the sopor meanwhile being very decided, and the patient acting much as if under the influence of moderate general anæsthesia. At the expiration of this time he had a slight seizure; one half hour later another, very mild; and thirty minutes after, on opening a door to get into the outer air, he experienced one of considerable severity. The drowsiness continuing he was sent to bed, and on visiting him two hours subsequently, we learned that he had passed through two paroxysms, one slight, the other somewhat severe. The numbness of the lip and cheek was marked, and the drowsiness continued. This was 5 p.m. of the 22nd, from which date until the afternoon of the 27th he remained entirely exempt from pain. Dr. Mattison had withdrawn the morphia, the use of which had been uninterrupted for six years, and instituted treatment with a view to relieve the patient of the habit.

“On the 27th the patient, in washing his face, experienced a paroxysm of trifacial pain of decided severity, which continued three minutes, but without any of the spasmodic muscular movements attending the previous attack. Two hours later he had another seizure, of diminished severity, since when until the date of this communication, a period of nearly four weeks, he had been entirely free from suffering.”

The doctor, in concluding his report, says: “I may add, the

man has had no return of his *tic*; his reformation as to the opium habit seems complete, not having had an atom of morphia since the chloroform injection. He has been entirely exempt for weeks (something unprecedented during fifteen years' experience) from his sciatic trouble, making in all a very interesting case, with an eminently satisfactory result."

Although the encomiums (which I do not quote) bestowed by Dr. Mattison on the method of deep injection of chloroform may not be entirely just, it must be conceded that any remedy which relieves, even for a time, so intractable and horrible a disorder as *tic douloureux*, is certainly a very desirable addition to our medical armamentarium. That a permanent cure of *tic douloureux* can be wrought by this means of treatment, or that a case dependent on intracranial lesion can be relieved for any considerable period, it would be extremely rash to assert in either case.—*Practitioner*, July 1874, p. 9.

20.—NOTES ON THE NITRITE OF AMYL.

By Dr. J. CRICHTON BROWNE, F.R.S.E., Medical Director,
West-Riding Asylum.

In June, 1873, when administering the nitrite of amyl to a girl who was in the *status epilepticus* and had been unconscious for some hours, I was much struck by the fact that in about a minute after the inhalation was commenced there was what may be called an attack of yawning. The patient yawned profoundly and repeatedly. Never having before witnessed yawning during a state of coma, it at once occurred to me that that modification of respiration in this case must have been induced by the nitrite of amyl, which always when inhaled hastens and deepens breathing. That inference I was able to corroborate forthwith. Whenever the inhalation was interrupted the yawning ceased, whenever it was resumed the yawning recommenced. In another case of the *status epilepticus*, which occurred about a month afterwards, similar phenomena were observed. The patient, although so completely comatose that no responsive movements followed upon tickling the soles of the feet or pricking the toes, immediately yawned in the most ordinary way, as if just upon the verge of much-needed sleep, whenever a piece of lint soaked in nitrite of amyl was held before the nose and mouth. When that was done, two or three full inspirations were succeeded by one of extreme depth, accompanied by depression of the lower jaw and elevation of the ribs and scapulæ. The experiment was repeated many times, until it was quite evident that the crowning expression of drowsiness was induced by the nitrite of amyl. Some months

later, Dr. Herbert Major again noticed yawning in a general paralytic patient, who was quite unconscious and at the point of death, and to whom he was administering the nitrite of amyl with the view of securing a temporary rousing. More recently, on the 24th of July last, the same effect of the nitrite of amyl was seen well exemplified. Two male patients were at that time prostrated in the *status epilepticus*; one of them, M. C., had passed through some hundreds of fits in the course of three days, and was permanently unconscious and much exhausted; and the other, J. A. M., had had nineteen fits in twenty-four hours, and was in a condition of stupor, taking no notice of what was going on around him, and making no spontaneous movements. At 2 p.m., nitrite of amyl, which had been tried at the beginning of the outbreak without perceptible benefit, was again given as an inhalation to M. C., who was then *in extremis*, lying with livid features, pin-point pupils, a pulse of 116 and a temperature of 102°, and breathing stertorously. When the inhalation had gone on for fifteen seconds, there was a voluntary movement of the right hand and an attempt to raise it, and this was speedily followed by acceleration of the respiratory movements and then by distinct yawning. The patient yawned six or seven times consecutively and prodigiously, and then turned his head and emitted a kind of sob. While the yawning was going on the pulse was somewhat quickened, and a dull flushing of the head and face became visible. The flushing travelled about half-way down the neck, but not further, and no blotches appeared on the chest, abdomen, or limbs. Three minutes after the inhalation a copious perspiration burst out over the forehead, face, and neck. At the same time an inhalation was administered to J. A. M., who could not be awoke out of his heavy slumbrous torpor. In ten seconds he likewise moved his right hand, and in fifteen seconds he likewise began to yawn. It would perhaps be more correct to say that he manifested a tendency to yawn. He opened his mouth to the widest possible extent several times in succession, but with that movement there was no prolonged inspiration, no raising of the shoulders, and no characteristic sound. With the movements of the jaw came obvious flushing and considerable reanimation. The patient looked about him, lifted his head from the pillow, and seemed more alive to surrounding circumstances than he had been for twenty-four hours previously.

Two days subsequent to these observations on M. C. and J. A. M., my colleague, Dr. Merson, who had witnessed them, was administering the nitrite of amyl to a retriever dog that had been in continuous convulsion for some hours, when he was much surprised to notice in it precisely the same movements

that he had seen in J. A. M. Whenever the amyl was held before the nostrils of the animal, which at the time was quite unconscious, it beat the ground with its fore-paws, and opened and closed its mouth with rhythmic regularity. To make quite sure that these movements were not accidental or due to some other cause, Dr. Merson interrupted and resumed the inhalation many times. He found that invariably when it was resumed after an interruption these movements occurred, and that they were not induced by other kinds of stimulation.

These phenomena, as observed in the dog and in patients in a state of coma, seemed to me to be highly interesting, and to point to some hitherto unsuspected action of the nitrite of amyl. Having administered that agent to hundreds of patients in a state of consciousness, I had never seen yawning included amongst its effects; and having searched its literature, I found no record of any such action having been produced by it. That the yawning, complete or partial, in the cases described, and in the dog, was really ascribable to the nitrite of amyl, is evident, I think, from the nature of the observations made and referred to, as well as from the general observation that yawning is not known to occur in states of coma or unconsciousness due to disease in which nitrite of amyl has not been administered. Arising ordinarily out of a sense of fatigue or an oppression of the respiratory organs, it does not occur during morbid fatigue nor pulmonary engagement. I have watched innumerable cases of coma with great exhaustion, and every degree of lung congestion, and in none of them except those in which the nitrite of amyl was used have I seen yawning. That the yawning was due to some specific effect of the nitrite of amyl, and not to a mere general stimulation of the pulmonary mucous membrane under certain conditions, is deducible from the fact that other stimulants under like conditions do not produce the same effect. Thus chloroform, ether, and ammonia, given by inhalation in states of coma, do not bring on yawning as the nitrite of amyl does.

Curious to trace out further the action of the nitrite of amyl when inhaled during unconsciousness, I have during the past month administered it in this way to eighty-seven persons when sleeping, and to many of these upon several occasions. Great difficulties of course obstruct the attainment of any definite results in this way, as the most general and immediate consequence of the inhalation of the nitrite amyl during sleep is sudden waking. At the moment that the blush appears upon the face, the person operated upon starts up, makes a number of voluntary movements, and having passed through a few seconds of bewilderment, not unnatural after so rude and extraordinary an interruption of repose, is fully alive to all

that is going on around. With great care, however, and the use of small quantities of the nitrite, the inhalation may be carried out and its full effects developed during the continuance of sleep. My most successful observations have been made upon epileptic patients who sleep heavily and are not easily disturbed. Altogether I have been able to make fifty-seven satisfactory observations; that is to say, I have succeeded fifty-seven times in administering the nitrite of amyl without waking the patient, and in noting the effects which followed the inhalation. It will certainly be regarded as remarkable that in forty-two out of these fifty-seven observations there were recorded distinct movements of the mouth consentaneous with blushing of the countenance. In only one out of the forty-two observations in which movements of the mouth are reported did yawning occur. That observation was made upon a lad who was attacked by a severe fit when asleep, and to whom the inhalation was administered about five minutes after the fit, while his features were still livid and his breathing stertorous. No sooner had the flush overspread his face than he turned upon his back and yawned to the top of his bent five times, then falling again into a heavy sleep. In none of the other forty-two observations is yawning recorded, but in all of them movements kindred to yawning were noticed; that is to say, in all of them there were movements of the mouth. These movements were exceedingly various, the most common, however, being a short munching movement of the lower jaw, which was depressed and elevated as if in the act of chewing. The next most common movement was a smacking of the lips, as if in the act of tasting. It will perhaps be as well to quote from my note-book a few illustrations of the movements observed. In twenty-eight of the successful observations, movements of the hands, antecedent to or contemporaneous with those of the mouth, were noted.

1. Mary C., epileptic, M 5. In twenty seconds, deepened breathing; in thirty seconds, distinct flushing; in thirty-five seconds, extension of both hands; in thirty-eight seconds, munching movement of lower jaw and movement in throat, as if swallowing was going on.

2. Ann W., epileptic, M 8. In ten seconds, deepened breathing; in fifteen seconds, slight flush; in twenty seconds, deep flush; in sixty seconds, munching movements of the lower jaw, kept up for thirty seconds, when sudden waking occurred.

3. Emily L., epileptic, M 10. In five seconds, deepened breathing; in nine seconds, flush, and the right hand suddenly raised above the head, after which munching movements of the lower jaw began.

4. Elizabeth W., epileptic, \mathfrak{M} 15. In ten seconds, quickened breathing; in twelve seconds, movement of the right hand; in fifteen, distinct smacking of lips, continued for some time.

5. Jane A., epileptic, \mathfrak{M} 5. In twenty seconds, deep flush, then short cough, then smacking of the lips.

6. James G., epileptic, \mathfrak{M} 10. In ten seconds, flushing of the face; in fifteen seconds, retraction of the lips, then loud grinding of the teeth.

7. Paul G., epileptic, \mathfrak{M} 10. In ten seconds, deepened breathing, becoming audible and stertorous, then movements of the lips, then swallowing, and then muttering and mumbling.

8. Job L., epileptic, \mathfrak{M} 5. In ten seconds, deep flush, with blowing movements of the lips, followed by movements of the hands.

9. Thomas H., epileptic, \mathfrak{M} 5. In five seconds, deepened breathing; in ten seconds, smacking of the lips, with movements of the hands, after which he turned over in bed.

10. Thomas W., epileptic, \mathfrak{M} 5. In ten seconds, hand raised to head, over which it made a rubbing movement; in fifteen seconds, munching movements of mouth.

The same patients in whom these movements were observed were also caused to inhale during sleep ether and aromatic spirits of ammonia, with the view of testing whether other stimuli applied to the respiratory tract would induce like movements. It was found that ether, cautiously administered, merely deepened sleep, without producing any movements whatever; and that when movements were occasioned by it, these usually commenced round the eyes, and not the mouth. Ammonia always awoke the patient, the first movement being a sudden and violent toss of the head backwards.

As the result of these observations, it appears that nitrite of amyl, when inhaled during a state of unconsciousness, has a specific action upon the motor centre of the mouth, and calls into action, by preference, the muscles of the lips and lower jaw. The mode in which it performs this action, whether reflexly or through the agency of the vaso-motor apparatus, is as yet only a subject of speculation. The fact that the movements which it evokes are consentaneous with the appearance of flushing of the face, gives probability to the latter hypothesis; while, on the other hand, one observation, that when administered hypodermically the nitrite failed to induce yawning in a case in which it had induced it when inhaled, seems favourable to the other view. But whatever may be the explanation of the action in question, there can be no doubt that it is a significant fact, and worthy of further investigation.—*Practitioner*, Sept., 1874, p. 179.

21.—ON HYDROPHOBIA.

By GEORGE FLEMING, Esq., President of the Central Veterinary Medical Society; Veterinary Surgeon, Royal Engineers.

[The following article is part of a review of Mr. Fleming's work by the Editor of the British and Foreign Medico-Chirurgical Review.]

The very name "hydrophobia" is calculated to lead to error, for although, in the human subject, the dread of water is one of the symptoms of the disease, owing to the spasmodic action of the muscles of the throat, yet in the lower animals, and especially the dog, it is by no means constantly or even generally present, as is amply proved by experience. The opinion generally prevalent that the dread of water is a pathognomonic symptom in the dog may lead, and has led, to the most serious consequences, and Mr. Fleming quotes a case related by Blaine, in which a London physician of eminence pronounced an opinion that a dog which had bitten three persons could not be mad because it was able to drink. Fortunately this opinion was not adopted, and the wounds inflicted by the animal were duly attended to by Blaine, and the wisdom of the precaution was proved by the result, for in five weeks a spaniel which had been bitten became rabid, and a horse which had also been bitten was likewise affected. Other examples are given, but the facts in this particular are now well known, and require little further elucidation.

Mr. Fleming is very emphatic in his statements on this point, and he tells us that—

"It is not true that a rabid dog is hydrophobous. Water does not inspire it with fear or horror, and when it is put before the animal it does not produce aversion. From the commencement to the termination of the disease there is no antipathy to water. The many hundred rabid dogs seen by Blaine, Youatt, and others, did not evince any marked aversion to that fluid; on the contrary, the animal is generally thirsty, and if water be offered it will lap it up with avidity, and will always swallow it at the commencement of the disease. When, at a later period, the constriction about the throat—symptomatic of the malady—renders swallowing difficult, it does not the less endeavour to drink, and the lappings are as frequent and prolonged as deglutition is retarded. Even then we see the suffering creature in despair plunge its entire muzzle into the vessel and gulp at the water, as if determined to overcome the convulsive closure of its throat by forcing down the fluid. . . . So little dread have the canine species of water that they will ford streams and swim rivers, and when in the ferocious stage of the malady they will even do this in order to attack other creatures on the opposite bank."

A painful case is quoted, on the authority of a French writer, in which a lady who owned a greyhound, which was in the habit of sleeping under the bed, observed one morning that the animal, in addition to tearing and gnawing the coverlet, *drank a larger quantity of water than usual*, though it ate little. The lady, alarmed at this change in its conduct, consulted a veterinary surgeon, who, however, did not find anything to cause anxiety; but on the next day the animal wounded her slightly at the end of the finger when she offered it some food, and the day after this event it died, and "*had never ceased*," says the narrator of the case, "*to drink very copiously of water until the end*." The lady was bitten on the 26th of December, and on February 4th, in the succeeding year, she was seized with hydrophobia and died on the 7th.

The symptoms of rabies in the dog as well as in other of the lower animals, as the horse, the cow, the sheep, the goat, the pig, and in poultry, are most carefully and elaborately described by Mr. Fleming, and if we pass rather lightly over this part of the subject it is only from want of space. As to the dog, which is the most frequent source of hydrophobia, it is clearly a matter of vital importance to determine whether in any given case it is the subject of rabies or not, as the lives of many human beings may depend upon the diagnosis, and it is almost needless to observe that popular views on this subject are often very erroneous, and the practices founded on those views not only erroneous but dangerous. It is shown, for instance, that the common notion of a dog being afraid of water when it is rabid is altogether a mistake, and it should also be mentioned that even violence is not necessarily characteristic of the disease, for there is a variety of it called "dumb madness," in which the poor brute is rather an object of pity than fear, but in which the danger is as great to man or other animals as in the "furious" variety. Among the most important symptoms so ably grouped together by Mr. Fleming it would appear that the sound of the bark is very characteristic, and, indeed, the author quotes M. Sanson as describing it by a musical notation; in "dumb madness," however, this symptom is of course absent. We may briefly summarise the indications of rabies in the dog by stating that the animal is at first morose, but fidgetty; that it has no dread of water, but on the contrary will greedily swallow it until the spasms of the throat prevent it from doing so; that it has a great desire to bite, and at first attacks inanimate objects, as wood, straw, &c.; that its bark or howl is altered in tone, and that its fury is vented especially on animals of its own species. The mad dog, if not killed, dies from paralysis or asphyxia.

We pass over the account of the symptoms in man, because,

although Mr. Fleming's description is very good, the disease is well known and is accurately described in medical books and periodicals, and the diagnosis in the human subject presents but few points of difficulty.

The morbid appearances observed after death in the lower animals are carefully considered, and are also figured by Mr. Fleming, who, however, observes, and we believe justly, that, as in other nervous diseases, the pathological changes observed in the lower animals and in man, when death has been caused by rabies and hydrophobia, are not generally at all in proportion to the severity of the symptoms observed during life; and he goes further when he states that, in many cases, it would be difficult to express an opinion as to the existence or non-existence of the disease in the lifetime of an animal, if the post-mortem appearances were alone to be relied upon. The phenomena which have been observed are chiefly those of congestion, either of the nervous centres, as of the brain and spinal cord, or of the alimentary tract, as of the fauces, the stomach, and the intestines. In man, repeated examinations have been made after death from hydrophobia, and nothing has ever been found which could satisfactorily determine the nature of the disease. The congestion of certain parts and organs, which has been observed, may, as Mr. Fleming remarks, be a consequence of the terrible disturbance of the nervous centres rather than a cause of this derangement, and the signs of inflammation are too rarely present to warrant the belief that this process has any essential connection with the phenomena. In a note at the end of the book, however, Mr. Fleming quotes some observations made by Dr. Clifford Allbutt at a meeting of the London Pathological Society, in which that physician describes certain pathological conditions of the nervous system after death from hydrophobia, and which he connects with the nervous disturbances observed during life.

The treatment of rabies and hydrophobia is in some respects unsatisfactory, because the disease, either in man or the lower animals, is necessarily fatal. Mr. Fleming does not pretend to have discovered any method of cure, and he condemns the conduct of those persons who, from credulity or ignorance, believe in the efficacy of remedial measures when once the malady has declared itself, and he is still more strong in his condemnation of those who, from interested motives vaunt the discovery of specific cures for hydrophobia. But while he admits, as all must do, the incurable nature of the disease, he, nevertheless, deprecates unnecessary alarm in the minds even of those who have been injured by the bites of mad animals, because it does not necessarily follow that hydrophobia will ensue in all such cases. The somewhat prevailing practice of immediately and

indiscriminately killing the animal which has inflicted the injury is obviously a foolish and mischievous one, because it is by no means always certain that the brute is really mad, and thus the human victim may be exposed unnecessarily to the torture of apprehension when, perhaps, no cause of fear exists. The advice given on this point by Mr. Fleming is that a dog suspected of or attacked by rabies, or one which has been bitten by a rabid animal, should not at once be killed and buried unless there is reason to suppose *that no person has been wounded by it*, but if a person has been bitten, then the animal should only be killed if the malady is undoubtedly present. If the case is only a suspicious one it is well not to kill the dog immediately, but, to keep it securely confined and to watch it carefully, so as to observe whether rabies really supervenes; the time required for the development of the symptoms is not long, and when they are developed the animal must be killed and buried. If, too, an animal suspected or affected with the disease escapes from its owner, or from any locality, it is the urgent duty of every one to warn the police and cause strict precautionary measures to be observed.

In the case of the human subject the local preservative treatment, which alone is assuredly efficacious, must be resorted to before the absorption of the virus, and, in order to be efficacious, this treatment must be prompt, and the poison must be removed by suction, squeezing, washing, and cauterisation. If the bitten part is within the reach of the mouth of the individual attacked, he should himself at once suck the wound vigorously, or allow a bystander to perform this duty; and it is to be observed that the danger of so doing is not very great, for, as is well known in the case of certain poisonous agents of animal origin, they are not absorbed by the digestive canal unless there be some abrasion on the mouth or lips. Expression, washing, compression, and cupping, may all be employed, but, useful as these measures are, they ought to be supplemented as soon as possible by cauterisation of the injured parts, either by a powerful heat or by chemical action. Mr. Fleming does not hesitate to recommend the hot iron as undoubtedly the most convenient and perhaps one of the most effective agents that can be employed to destroy the saliva and the tissues tainted by it, and the actual cautery has this further recommendation, that several articles in common domestic use will furnish the necessary instrument. Of the chemical caustics the most efficacious are the strong fluid acids, as the nitric, hydrochloric, and sulphuric; other caustic agents, as nitrate of silver, corrosive sublimate, and chloride of zinc; and strong alkalies, as strong ammonia, and caustic potash and soda. Excision, also, is a very efficacious measure, but the greatest care

should be taken that every portion of flesh likely to have been in contact with the saliva be removed. Mr. Fleming gives some very valuable and interesting tables, showing the number of cases where immunity has followed the bites of rabid animals treated by cauterisation, and of cases of death where no such precaution has been taken, and the results, as might be anticipated, are strongly in favour of the adoption of such prophylactic measures.—*British and Foreign Medico-Chirurgical Review*, July 1874, p. 51.

22.—SUCCESSFUL TREATMENT OF A CASE OF TETANUS.

By Dr. J. B. CARRUTHERS, Edinburgh.

The pathology of tetanus and the most effectual remedy for that disease are yet undetermined questions in medical science. The peculiar character of the disorder and the post-mortem examinations have revealed as yet almost nothing; consequently our treatment of it must be purely empirical. Many drugs of directly opposite action have occasionally been used with apparent success in some instances of this disease. In estimating the effect of remedies, their action should always be noted, and, when beneficial, persevered in and recorded. And as there may be some facts in the following very acute case, which recently occurred in my practice, which may prove interesting to the profession—some novelty in the treatment, and ultimately a good recovery,—I beg to offer a short account of it, as its publication appears to me to be as desirable as, on behalf of science, it may be interesting. Having previously been convinced of the therapeutic value of chloral in combating the involuntary movements of chorea, I in this case wish to allude to its action combined with bromide of potassium in tetanic spasms, and with astonishingly good effect.

The patient, J. H., aged fourteen years, a boy at school, of an active and restless disposition, had always enjoyed good health. In the end of the first week of April he got his little-finger bruised and lacerated from a piece of iron falling on it. The finger healed satisfactorily, the nail coming away afterwards. On the 18th April, he and a companion were attempting to scale a high wall; being unsuccessful in their efforts, his companion, with heavy nailed boots, mounted on the shoulders of J. H., and in consequence of the difficulty of ascending the wall, he trod severely on his shoulders—so much so that he complained more or less of pain there afterwards. Next day he was kicked on the back in an encounter with some other boys, but he was very chary about letting this out, as he rather got the worst of it.

On the 24th of April, the first day I saw him, he was very

feverish, a pulse of 100, with pain on pressure over both scapular regions, for which I ordered anodyne fomentations. On the following day the symptoms were much the same, he having passed a restless night.

On the morning of the 26th he was seized with tetanic spasms, and rigidity of the muscles of the whole body, with most acute pain over the entire length of the spinal column, so much so that the least pressure with the finger on it was almost unbearable, and in fact brought on opisthotonos. As I now viewed the case as one of considerable medical interest, I asked Dr. T. Keith, of Edinburgh, to see the patient along with me. At our interview the tetanic spasms were most severe, and as the spinal pain had, if anything, increased, we injected twenty minims of nepenthe into his back, and ordered a dozen leeches to be applied over the seat of pain, and ten grains of chloral, to be given every three hours in syrup and water. At 9 the same evening Dr. Warburton Begbie saw the patient along with us. The pain over the spine was now greatly relieved, but he was very restless. Pulse 130; temperature 102.2° ; his jaw closed; the muscles of his abdomen and legs very rigid; his face livid; his skin warm, and covered with profuse perspiration. We all viewed the case as one of great anxiety, both from its acuteness and suddenness of attack, as well as from the gravity of the disease. The treatment now was restricted to ten grains of chloral hydrate and twenty grains of bromide of potassium in syrup and water every three hours, and to be watched with great care and quietude.

I may here state that Drs. W. Begbie and T. Keith took special interest in the case, freely giving me their valuable opinions on the salient points as they presented themselves during the treatment.

On April 27th at 10 a.m., I saw the patient, and found he had had a very restless night; was scarcely a quarter of an hour quiet during the whole night. Complains of some pain in his back, and of the burning of his tongue from the chloral; nevertheless the medicine was given during this and the following day every two hours. Pulse 122; temperature in the mouth 102.1° . Swallows milk pretty well when the tube of the cup is pushed into the side of his mouth.

April 28th. 10 a.m.: Had four hours' sleep during the night. The muscles of his legs softer and the tetanic spasms not so violent. As the pain over his spine still continued, a strip of belladonna plaster was applied over its entire length. At the time of my visit, the effort of swallowing his medicine, the taste of which he dreaded, brought on a paroxysm. In the evening of the same day an enema of castor oil and warm water was given, and the action of chloral was now distinctly seen in the congestion of his eyes.

29th. 10.30 a.m.: His bowels acted satisfactorily from the enema; had some hours' quiet sleep during the night, and on the whole his symptoms were alleviated.

30th. 10 a.m.: Slept well during the night. Has still fits of spasms frequently, but they are less easily brought about. Pulse 108; temperature 101.2° .

May 1st. 10 a.m.: Passed a good night. The muscles of the legs now much relaxed, but the rigidity of the recti abdominis and masseter continues. At 5 p.m., as Dr. W. Begbie and I entered the room, he had just awoke out of a sleep. The start at our entering brought on a spasm, which soon passed off, and he took a good drink of milk from the cup, which I may here state was his chief food during his whole illness.

2nd. 10 a.m.: Gradually improving. Slept well during the night. Can open his mouth about a quarter of an inch to-day. Has occasional attacks of pain and spasm, which only lasted a minute or two.

3rd. 10 a.m.: Newly awoke from sleep. Was very excitable and talkative, and replied, in answer to my question, that he was perfectly well, except the pain in his back. Pulse 100; temperature 101° .

4th. Progressing satisfactorily. At 5.50 p.m. Drs. Begbie and Keith expressed themselves satisfied with his general improvement.

5th. 10 a.m.: Had a good night's sleep. Pulse 96; temperature 101° . About 7 p.m. I was sent for hurriedly, as he had a severe tetanic spasm affecting his throat on awakening from sleep. When I saw him he had marked opisthotonos, and as it passed off he suffered from laryngeal symptoms of dyspnoea with phlegm, which were very distressing.

6th. Passed a restless night, and had several tetanic seizures. At our interview in the evening he was so much weaker from the return of the spasm that we thought it advisable to give him a tablespoonful of port wine every three hours.

7th. 10 a.m.: Had a good night's sleep, and is really well, only slightly weaker.

8th. 10 a.m.: Had several hours' sleep during the night and occasional spasms. In the evening he was again seized with laryngeal spasms with dyspnoea, which soon passed off after he had ejected some phlegm.

From this date the patient gradually progressed towards recovery. On the evening of May 10th the spasms were so mitigated, and the action of the chloral was so marked in his system, that we agreed to stop it, only giving the bromide in ten-grain doses every three hours, which we gradually diminished to three times daily till we discontinued it also. On the 18th

May he was able to open his mouth, put out his tongue for the first time, and eat a biscuit and some meat, which he relished.

The noteworthy feature in the treatment of this case is the quantity of chloral taken by the patient, he having taken 1140 grs. in sixteen days (equal to fully 71 grs. a day) in a most acute attack of tetanus, with the result of the spasms leaving him on May 12th, exactly eighteen days from the date of seizure; while in their place the peculiar action of the medicine showed itself in a variety of ways. All kinds of delusions ensued. The boy often imagined he saw strange objects in the room, such as carriages, ships, robbers, &c. He was even allowed to keep pistols beside him to quiet him. At one time he was talking, at another time smiling, at another time crying, and at another time fighting, &c. His mind wavered very much, so much so that he did not know his father. When told that his father was beside him, he said, "That man is an impostor, and will be punished; he has just come from a railway station, and I would like to shoot him." At another time he started up at the sight of his father, and called him "Mr. Pretender." One day he called out, "There is a ship sailing round the room with cream and biscuit," and having recently eaten a biscuit he asked for another to keep it company. Another day he imagined he was fixed in a tree with his head stuck in the branches. The same day he was engaged fighting a battle, and said he would not leave till the last, and cried out, "Save that boy; they are kicking him too severely." Often he could not be persuaded he was in his own room and bed, and continually exclaimed, "Carry me home—carry me home to my own bed." Another day he fancied he saw his father's master enter a carriage with liveried servants, and cried out, "He must have been left a fortune lately, from the number of horses and carriages he has." One day he thought he was swimming in the Firth of Forth to the island of Mickery, and begged of his aunt to blow up the swimming bladder under his arms, otherwise he would sink. A few minutes afterwards he said it was all imagination. The same day he said he was sailing in the boat to Fife to visit his friends, and remarked what jolly fun he would have. He imagined various characters were in his room; but latterly he often wept bitterly at being so foolish as to believe his own imagination. It is perhaps not unworthy of record that as his delusions lessened he seemed to remember afterwards what he saw and did, and wept nervously on that account. A susceptibility to form impressions, and a quickness and fertility of imagination, appeared to me to characterise all his actions. These delusions continued in many forms and varieties, but gradually lessened, till the 25th of May, when they finally ceased, and the patient is now convalescent.

The principal point of interest in this case is its acuteness, accompanied as it was with severe spinal pain. Perhaps it may be a matter of opinion what was the primary exciting cause of the tetanus. Undoubtedly it was traumatic, and probably due to the bruised little finger. But it appears to me that the attack of acute spinal disorder from which he suffered at the commencement of his illness could only have resulted from one or other of the injuries to his back.

As regards the treatment, the marked amelioration of the symptoms on the third day after the chloral hydrate and bromide of potassium were given shows considerable efficacy in favour of these drugs in subduing the spasms. At the first the case was as disheartening as any case could well be, but by steady perseverance in the treatment the convulsions gradually weakened until they altogether ceased, and, finally, the patient got well. Although in some particulars this case may deviate from the usual professional groove of practice, I think it should furnish an interesting and not unuseful page in the history of chloral in connection with the treatment of that formidable disease, tetanus. I was much struck with the remedial effect produced by it in my little patient, whose life at one time was in extreme danger. As narrated by Dr. Reynolds, "the mind wanders much after a single large dose of chloral," and true to this peculiarity of the medicine in my case, where the quantity administered was very large, though cautiously and gradually administered, the delusions were in proportion multiplied and intensified, thus plainly establishing both the physiological and therapeutical action of the medicine.—*Lancet*, September 26, 1874, p. 445.

DISEASES OF THE ORGANS OF RESPIRATION.

23.—ON "REST" IN THE TREATMENT OF PULMONARY PHTHISIS.

By Dr. FREDERICK T. ROBERTS, Assistant Physician to University College Hospital, and to the Brompton Consumption Hospital.

[The idea which seems to prevail among the members of the profession generally is, that phthisis is always a constitutional, tubercular affection. This idea is quite erroneous; with reference to it, Dr. Roberts observes:]

A tolerably extensive experience has thoroughly convinced me of the following facts:—1. That pulmonary phthisis is, in a large number of cases, entirely local in its origin, having nothing whatever to do with tubercle or with any constitu-

tional taint. 2. That in many instances where tubercle is found in the lungs at the post-mortem examination, it is the result of local irritation or local infection, due to some pre-existing morbid condition. 3. That where the formation of tubercle is the primary morbid condition, much of the subsequent destruction of the lung-tissue is the result of the inflammatory products to which it gives rise by its irritation. Were phthisis, in its commencement and progress, invariably a local manifestation of a constitutional disease, the advantages of rest would certainly be very limited; but granting it to be a local affection in a good proportion of cases, and this is much more likely to become an important element in its treatment.

The questions relating to the advantages to be derived from rest in the treatment of phthisis, either preventive or curative, may be discussed under certain headings.

1. *General rest of the body, and restriction of the respiratory functions.*—In every case of established phthisis, and also where there is a predisposition to this disease, it is of much consequence to determine carefully whether it is desirable for the patient to take exercise or not, and if so, to give as definite instructions as circumstances permit or require, with regard to the kind and amount to be indulged in, and particularly as to the degree to which the respiratory organs are to be called into action. But too commonly this matter is ignored altogether, while in other cases indiscriminate exercise is ordered, and that not seldom of a violent kind, where the indications point decidedly in the direction of rest; or, on the other hand, a patient is kept in the house or in bed who would be much the better for some out-door exercise. On this point each case must be considered on its own merits, but there are certain general indications which may, I think, be taken as guides, to which I will now briefly direct attention, first referring to those cases in which there is merely a danger of phthisis becoming developed; and, secondly, to those in which it actually exists.

While holding to the *local* origin of phthisis in a good proportion of cases, I do not for one moment question its *constitutional* origin in many of them, and it is about those cases in which there is a supposed constitutional tendency to consumption, either hereditary or acquired, that I wish to speak here. In the first place, wherever there is at all a marked hereditary predisposition to phthisis, if circumstances permit, it is very desirable that the person thus predisposed should be intelligently guided in the matter of exercise, as well as with regard to other measures having reference to the maintenance of health, especially during the period of growth and development. This does not mean that we should always be laying down positive and

exact rules as to what must and what must not be done, but merely that we should exercise a general supervision over these patients. Taking the bulk of individuals hereditarily predisposed to phthisis, it may be stated as a general rule that they are quite capable of taking a fair amount of ordinary exercise, such as walking or riding, and need it too, while they are certainly the better for being out of doors and inhaling as much fresh pure air as they can get. If these patients are evidently delicate and weakly, the amount of exercise must be limited to suit their capabilities, and it is this class of patients who chiefly need to be guided.

Now there are two opposite errors which are often committed in the cases at present under consideration, and which need to be guarded against. On the one hand, these patients not uncommonly suffer from *deficient* exercise, especially during the periods of youth and adolescence. Their friends are, in not a few instances, under the impression that they are too delicate to undergo any exertion, and indulge them in idle, enervating habits, which are decidedly injurious to them. This is particularly likely to happen if the patients themselves feel languid and are easily fatigued by a little effort, their sensations being but too readily accepted as a correct indication of what they need in the way of exercise. Indeed, experience has taught me that it is no easy matter to make people comprehend the essential importance of out-door exercise in many cases of this kind, but when they do carry out the advice they usually soon recognise its advantages. Then, again, many persons predisposed to phthisis are seriously injured from want of exercise in connection with their occupation. I do think that a great deal more consideration should be given to the finding of suitable employments for such persons than is customary, not only with reference to exercise, but also to other sanitary matters. What can be expected, when they are made to follow some calling which keeps them confined to a room or office for twelve or more hours daily, deprived of all out-door exercise, and generally surrounded by anything but satisfactory hygienic conditions, but that the constitutional disease will manifest itself, especially if this happens at a tender and critical period of their life? Yet this is a true representation of the case of many of the patients with a hereditary tendency to phthisis, who present themselves in hospital and private practice. I say emphatically therefore, that if such a state of things comes under the notice of a practitioner, one of his first duties is to insist upon an immediate change to some more satisfactory employment.

The other mistake which calls for notice tends in a contrary direction to that just considered. It is not unfrequently the

custom to recommend patients predisposed to consumption to practise various exercises with the view of expanding their chests. To do this without proper discrimination is decidedly wrong and may lead to very serious consequences. All violent exercises which bring the lungs into forcible play, such as running, rowing, gymnastics, &c., are dangerous in such subjects, even when they are apparently strong and well, but especially if at all delicate, and therefore need to be indulged in cautiously. Not a few cases have come under my notice in which active disease seemed to have originated in a rupture of a blood-vessel in the lungs produced in this way. Then, again, most of these persons have chests of very fair size, which do not particularly require to be enlarged. If all forms of the disease are included, my experience at Brompton Hospital has taught me that the great majority of phthisical patients can compare not unfavourably with other individuals as regards the shape and dimensions of their chests, and this is true also of a large proportion even of cases of constitutional origin. I do not, however, by any means intend to imply by these remarks that all persons exhibiting any predisposition to phthisis must be prohibited from all exercises of this kind. They are often beneficial, if indulged in moderately and with care, and if the chest should be small in any person, whether predisposed to consumption or not, various methods for expanding it may be employed with much advantage, especially in early life, provided they are carried out judiciously. Among other methods, I think a useful and safe one is to instruct the patient to go through a process of *deep breathing* for from two to five minutes every morning and evening, inspiring and expiring forcibly from twelve to fifteen times in a minute.

Coming now to those cases in which there is *actual pulmonary* mischief, I remark first that under such circumstances *all* forms of violent exertion, but especially such as call the lungs into undue play, are decidedly to be avoided, as they are liable to lead to serious harm. On the other hand, the great majority of cases of phthisis are improved by more or less walking exercise, not too vigorous, provided the conditions of climate and weather are suitable. The exact amount of such exercise to be recommended must be guided by the local and general conditions observed in each patient. When they can be indulged in, quiet riding and driving are beneficial, the latter being also very useful for those who cannot take active exercise. I am sure that many phthisical patients are injured by being kept too much at rest and indoors, or at least they are not sufficiently encouraged to go out and walk. The want of out-door exercise is one reason why the progress of many consumptive patients who go into general hospitals is anything but satisfactory. One

great advantage also of sending such patients to the more genial climates is, that it enables them to go out, when otherwise they would be obliged to remain in the house. There are conditions, however, in which it becomes very important either to limit the amount of exercise considerably, or even to enjoin perfect rest for a time. Thus, exercise must be restricted if the disease is extensive or spreading rapidly; if it is decidedly of a tubercular nature; if there is a disposition to much pyrexia; and especially if, with either of the above conditions, any little exertion brings on much dyspnœa, cough, or hæmoptysis, or causes much sweating with a sense of great debility and fatigue. I particularly wish, however, to draw attention here to those cases which have a more or less *acute* origin. I believe it is extremely important that in any case beginning in this way, whether due to pneumonia, catarrh, tubercle, or whatever else it may be, the patient ought to be kept completely at rest, or even confined to bed. Not a few cases go from bad to worse either because they are not able to take the needed rest in the early stage of the disease, or because they are not instructed to do so. Hence it is a great advantage to get patients into hospital at this stage, where they can be kept at rest, and at the same time receive proper treatment of other kinds. Rest is also imperatively called for if *hæmoptysis* of any amount occurs, whether there is actual disease in the lungs or not, and even though the patient present no constitutional tendency to phthisis. It is also important if in the course of a case *acute exacerbations* should arise.

2. *The conditions of the air breathed.*—There can be no doubt but that some forms of phthisis are directly due to the habitual inhalation of air of irritating quality, especially such as holds solid particles in suspension. In a less degree it has an influence in the causation of very many other cases, as when patients are obliged to work in confined and impure atmospheres, or are exposed to cold and damp. It is obvious that when such a case comes under treatment, the first thing to be done is to remove the patient at once from conditions which are so evidently injurious, and thus give the lungs the rest which they need from the constant irritation to which they are exposed. If this is not done, not only is a cure impossible, but the disease must necessarily progress. Unfortunately patients are often prevented by circumstances from acting upon this advice, but still it should be urged as one of the most important elements in the treatment of such cases.

The effect of any kind of irritating atmosphere in exciting cough must also not be forgotten. Many phthisical patients complain that when they go out into a cold and damp or foggy atmosphere, their cough becomes very severe, and of course

they should be warned against exposing themselves to these evident causes of irritation. If circumstances compel them to do so, they should wear respirators. The same applies to every condition in which the air is likely to cause irritation.

3. *Suppression of cough and other violent actions connected with the respiratory organs.*—Space will not permit me at present to enter upon my reasons for making the statement, but I have a strong belief that *cough*, especially if *severe and long-continued*, may be the *immediate cause of pulmonary phthisis*, no matter what such cough may be due to, even though it is only the result of some irritation in the throat. Hence it is, in my opinion, of great importance not to allow cough to continue for any length of time, however trivial the cause may appear to be. This applies to all individuals, but especially to those who are predisposed to consumption. Should the cough be associated with some evident pulmonary complaint, even if it is only a little bronchial catarrh, and particularly *if the apices of the lungs are affected*, the checking of long-continued cough is still more important. When phthisis is actually developed, the management of this symptom is often a matter of much difficulty. It becomes a question whether and how far it is desirable to restrain it, but it may be laid down as a general rule, that if the cough goes beyond what is necessary for getting rid of the materials formed in the lungs, it ought to be checked, and especially if it is of an irritating character, and comes on in severe, irrepressible fits. It may then do a great deal of harm if allowed to continue, by tending to increase the mischief in the lungs; by causing serious vomiting of food; by exhausting and weakening the patient very much; or by leading to some direct lesion in the lungs, such as the rupture of a blood-vessel or the bursting of a cavity with consequent pneumothorax.

It is beyond the province of this paper to allude to all the remedies which may be used to relieve cough. In practice it is often found by no means an easy matter, and one has to vary the measures employed very frequently. Various anodynes and sedatives are of course useful, especially morphia in small doses. I would draw special attention, however, to the necessity of always looking to the state of the throat and larynx, as some cause of irritation is often found here; and also of instructing the patients not to force themselves to cough, as many of them are in the habit of doing, with the view of bringing up the phelgm.

As regards other actions in which the lungs are unduly exercised, such as speaking in public, singing, &c., it is only necessary to say that these ought to be forbidden if the lungs

are actually diseased, or if there is evident danger of their becoming affected.

4. *Applications of mechanical apparatus over the chest-walls, with the view of procuring local rest.*—It is to this particular mode of giving rest to diseased lung-tissue that Drs. Berkhart, Dobell, and M'Crea of Belfast have alluded in their papers on the subject, and I desire to add the results of my own observations in the same direction. That this mode of treatment is very valuable in certain cases and for certain purposes, anyone who practises it will soon find out. And I would remark, by the way, how difficult it is to impress upon many minds the advantages of simple, common-sense measures, however obvious these may be. This seems to me to be a measure coming under this category, but very few take much notice of it. I strongly urge, therefore, upon all practitioners, that at least they should not ignore it altogether because of its simplicity, but see whether it may not be of some service to them in the treatment of cases which are anything but easy to manage as a rule.

Beginning with the form of apparatus to be employed, I think that every advantage which can be expected may be derived from strapping the chest with strips of some plaster spread on a firm material. There are positive objections to the use of any elaborate form of "lung-splint," and I do not think such an instrument is at all needed. The plan I adopt is very much like that described under "Pleurisy," except that it is only desirable to apply the plasters over a limited portion of the chest, and that in most cases it is the upper part which has to be fixed. When this is required, I practise a plan very similar to that described by Dr. M'Crea in this week's *Lancet* [see p. 103], passing vertical strips firmly over the shoulder from behind forwards, and others round the side. The differences are, that I use broader strips; apply those round the side, some obliquely, crossing each other, others horizontally; and if there is any marked depression above or below the clavicle, I fix pads of cotton-wool in the hollows, beneath the plasters.

Let us now consider for what purposes and in what conditions this form of treatment may be of service. First, it may be employed with the view of promoting *curative* changes, and *preventing* the spread of the morbid process to healthy portions of the lungs. For this purpose it is particularly serviceable in those cases where the disease is limited to *one apex*, especially if it is of a chronic nature, or if curative changes have commenced and are advancing. Thus I feel sure that in several cases where a cavity at one apex has begun to contract, the healing process has been aided by the use of plasters. So, too, when acute pneumonia involving the apex of one lung has originated phthisical destruction, I have found the same appli-

cation beneficial, as well as in a few exceptional instances where this disease has led to the same result at one base. Where both apices are affected, the application may be safely made to one of them, and sometimes with advantage, especially if the healing process has commenced there, and if the disease does not appear to be making progress. As a general rule it may be stated, that the more evidently the lung-disease is of local origin, the more limited it is; and the more clearly it appears that all active mischief has ceased and that curative changes are going on, the better is the prospect from the use of mechanical applications to the chest as a means of cure, or of prevention of further extension of disease. If the complaint is decidedly tubercular and constitutional; if it shows signs of spreading rapidly; if there is extensive disseminated disease through one lung, or *à fortiori* through both, but little, if anything, can be expected from these applications for the objects above mentioned.—*Practitioner*, Aug. 1874, p. 99.

24.—ON STRAPPING THE CHEST IN PHTHISIS.

By Dr. JOHN MCCREA, M.A., Medical Officer to the Belfast Dispensary.

The treatment of phthisis by restraining chest-movement deserves more attention than it has yet received. Partly for this reason, and partly to describe the appliance which I have latterly found most effective, I wish again to direct inquiry to the subject.

In the large number of cases which have come before me in the practice of the Belfast Dispensary, I have seen no remedy equal strapping the chest in efficiency and general applicability. At the same time the use of other remedies is not interfered with. The plasters used in strapping are quite able to bear the strain of walking and talking, so that gentle exercise and conversation are not forbidden; and, indeed, I have seen both rendered enjoyable where they had previously been irksome. I have not met with a case in any stage of the disease in which there was ground for attributing any bad result to the restraint of the chest. I say this because a paper on the subject threatened grave consequences if cases were not most thoughtfully selected after an exact measurement of the proportion of lung involved. An extensive trial has convinced me that this dread is a dream and this refinement finical.

Since writing a paper which appeared in the November number of the Dublin Journal of Medical Science, I have made an improvement in the apparatus, which diminishes the frequency of the renewal of the plasters and strengthens their grip. The following description contemplates their application

to the upper part of the chest. I have principally used emplastrum roborans spread on swans down. The sheet, which is half a yard wide, is to be cut into transverse strips. Each strip is eighteen inches long; the breadth should be about three-quarters of an inch. The plasters should be only very slightly heated. The first strip runs up the back in the space between the spinal column and the posterior border of the scapula on the affected side, its starting-point being well below the level of the inferior angle of the scapula. It is to be applied gradually and deliberately, every portion being well rubbed in before the next portion is brought into contact with the skin. It is to be carried over the shoulder and down the front of the chest. In rounding the shoulder it is to be pulled tight and held so while it is being, bit by bit, brought into contact with the front of the chest, the chest just at this period being in the act of strong expiration. The next strip, which is horizontal, commences at the spine, crosses the posterior end of the first strip, passes under the axilla and on towards the sternum. It also is to be applied deliberately and with friction; as it is rounding the chest it is to be pulled tight, the patient at the same time making a forced expiration. Other strips are to be applied in a similar manner, vertically and horizontally time about, until it is judged that a proper grasp of the chest has been obtained. I avoid the scapula as much as possible. Some of the horizontal strips should cross the sternum, and some the spine. A large rectangular piece of plaster should now be applied, occupying the interscapular space and reaching down to the last dorsal spine. Another squarish piece is to cover the front and upper part of the chest between the clavicles and mammæ. These, if smoothly applied, secure the ends of the strips from ruffling up, and give additional *points d'appui*. Finally, the whole is to be well rubbed in all over. The patient is to sit quiet for a few minutes before dressing. The plaster soils the fingers, which, however, may be easily cleaned by rubbing with coarse paper and washing with a few drops of ether. The length of the strip required of course depends upon the size of the chest and the extent of the disease. I always endeavour to control more of the lung than the portion apparently diseased. I have found it generally suitable to cut the plaster as above described. If too long, that may be easily remedied with scissors as each strip is applied. If too short—if, for instance, a vertical plaster beginning on the back does not reach sufficiently far down the front of the chest, let the next vertical plaster commence its course in front and at a sufficiently low point, and then be made to cover the former. This, besides, increases the rigidity of the apparatus, and rigidity undoubtedly is one source of its power.

In a fortnight a reapplication will probably be required. This will give a good opportunity for a careful examination of the condition of the lung. While the plasters are still on the indications of the thermometer will be most valuable. If there be an exacerbation of the symptoms, particularly of the cough, dyspnœa, or pain, if the temperature rise, or if the plasters be obviously slack, apply new ones. In an advanced case of phthisis in a girl, the girl's mother told me that she herself could tell the proper time for renewal by observing the cough become distressing at night; and, indeed, it is common for patients to ask for a reapplication. This illustrates, besides, the confidence felt in the plasters by those who have had experience of their effects. In early phthisis it is necessary to warn the patients not to mistake the amelioration of their symptoms for recovery; they should always be directed to come back. Possibly when they consider themselves quite well the thermometer or the stethoscope will indicate differently. These are the cases in which, by reapplications, repeated reapplications if necessary, we may hope for the most brilliant results.

In the paper already referred to I have related a few cases, selected with the aim of illustrating the effects of this line of treatment in different stages of the disease. We obtain immediate and marked diminution of the cough, cessation of pain, relief of dyspnœa, and reduction of temperature; and the patient usually expresses at once a feeling of great comfort. In short, I am so satisfied with the results of the numerous cases in which I have tried this method that I give it the first place among all the remedies for phthisis.

Papers on the subject.—Berkart on "Rest," &c., *Lancet*, Oct. 18th, 1873; a letter from myself in the following number of the same journal; my paper in the *Dublin Medical Journal*, November, 1873; Dobell on the "Importance and Dangers of Rest in Pulmonary Consumption," in the *British Medical Journal*, November 22nd, 1873.—*Lancet*, July 18, 1874, p. 76.

25.—ON "REST" IN THE TREATMENT OF CHEST AFFECTIONS.

By Dr. F. T. ROBERTS, Assistant-Physician to University College Hospital, and to the Brompton Consumption Hospital.

Pleurisy.—Before proceeding to consider the advantages to be derived from rest in certain other pulmonary affections, I may just mention a case which is at present under my care at the Brompton Hospital, and which seems to me to indicate very clearly the beneficial results to be derived from strapping

the affected side in cases of pleurisy, even when there is a considerable amount of fluid effusion. The patient is a woman, aged 34, with a marked hereditary phthisical history, and she has suffered from the local and general symptoms of pulmonary phthisis for some years, but lately had been much better. She is supposed to have caught cold, and a return of the chest symptoms resulted from this, with a severe stitch in the side. When she came to the hospital, she presented an aspect of serious illness, with much emaciation. Breathing also was much distressed. On examination of the chest, the remains of old disease were found at both apices, but in addition there were the signs of considerable pleuritic effusion on the right side—so much, indeed, that I feared fixing the side could be of very little service. However, I resolved to try the effect of this treatment, and it has proved most satisfactory. The pain and distress in breathing ceased immediately the application was made, and the patient, on each occasion that she visits the hospital, alludes with gratitude to the marked relief she experienced.

But what is more important is, that absorption commenced speedily, so that in a week the fluid had greatly diminished in quantity, and at the end of a fortnight it was entirely removed. I have a strong conviction that strapping the side did in this instance check the effusion, and hasten and aid its absorption very materially. There was nothing in the internal medicines employed which could have at all promoted this object. I may add that the patient's general condition has improved considerably.

There are two other affections in which mechanical applications to the chest prove very serviceable, and these demand a brief notice.

Pleurodynia.—This complaint, which is often a very painful and annoying one, may generally be relieved completely by applying two or three strips of plaster firmly round the side over the seat of pain. The emp. roborans is a good form of plaster for these cases. It is desirable that the practitioner should himself see that the application is properly made, and that it is not left to the patient, as the result entirely depends on its being accomplished in an effectual manner.

Pneumothorax.—My own experience of this morbid condition, in medical practice, is limited to those cases in which a cavity in the lung has given way in the course of phthisis, and consequently air has escaped into the cavity of one of the pleuræ. Two classes of cases occur, differing materially as to their severity and importance. (a) In one set, owing to the existence of extensive adhesions or agglutinations in connection with the pleura, only a limited quantity of air escapes,

and the accumulation is confined within a small space, so that respiration is not materially interfered with, and the chief symptom is the pain in the side, which has come on suddenly and is generally very considerable. Physical examination reveals the signs of local excess of air. In these cases I have found satisfactory strapping of the side give immediate relief; while as a rule no further ill-effects were experienced from the mishap, the air, I suppose, becoming absorbed, and the opening from the lung closed. (b) In the other class, which are far more grave, adhesions either do not exist at all, or they are insufficient to prevent the air from pervading the entire pleural sac, which therefore becomes more and more distended, the chest-wall being driven out, and the lung compressed. Under these circumstances breathing becomes greatly embarrassed; and what is often very distressing to the patient is, that there is a feeling of great distension and want of support over the side, with much difficulty in coughing. In cases such as this the application of some mechanical apparatus over the affected side is eminently serviceable, especially if employed at an early period after the perforation has taken place. This apparatus needs to be very firm and resisting, and therefore the best form is that of plaster, covered over with two or three layers of strips of bandage steeped in a mixture of gum and chalk, as described when speaking of pleurisy. Its effects are, that it limits the escape of air, if applied soon, by restraining the respiratory movements; or, if much air has already accumulated, it gives considerable relief by affording a sense of support over the side, rendering breathing more easy, and enabling the patient to cough more effectually. Should the pleural sac be much distended, however, the best plan of treatment would be to remove the air from the pleura by means of the Aspirateur, and then immediately put on the apparatus I have mentioned.—*Practitioner*, May 1874, p. 329.

26.—CASES OF ACUTE TUBERCULOSIS.

By Dr. J. W. F. SMITH SHAND, Physician to the Aberdeen Royal Infirmary.

The following cases are examples of acute miliary tuberculosis occurring in persons who had previously enjoyed good health, and who succumbed to the disease in the course of a few weeks. The supervention of acute tuberculosis in cases of chronic wasting disease is not uncommon, and the probability of its occurrence may be anticipated; but the other variety is more rare, and the difficulties experienced in the diagnosis are often very great. In itself a constitutional disorder, it often

commences with the symptoms of a common cold; subsequently it may assume the characters of a specific fever—an enteric fever; and then, even with the aid that the thermometer affords, the practitioner finds himself oppressed with doubts as to the real nature of the case.

The duration of the disease and the symptoms in tuberculosis and enteric fever are so much alike that it is probable the one is not unfrequently mistaken for the other, and that the rarer disease is set down as an example of typhoid fever presenting, it may be, supposed anomalous symptoms. In other cases it is apt to be mistaken for capillary bronchitis, or even meningitis.

Case 1.—W. G., aged seventeen, a farm servant, was admitted into the Aberdeen Infirmary on April 13th, 1864, complaining of cough and weakness; no expectoration. His face was of a dusky hue, and there was great expansion of the nostrils during inspiration. There was also considerable aphonia, but he made no complaint of pain in the larynx, or in any other part of his body except the small of his back. On inspection the body was slight and spare, and the chest well formed. The respiratory movements of the thorax were much exaggerated and quickened, and, on applying the hands, rhonchial fremitus was extremely well marked. Percussion elicited a clear sound over both lungs. Auscultation gave fine dry sibilant rhonchi equally over all the chest, but they were not very intense. Heart sounds normal. Pulse 120. Tongue moist, with a light-brown fur. Bowels regular. Urine 1025; gave no precipitate with heat or nitric acid. Burning heat of skin.

He stated that his present illness came on with a cough a fortnight before admission, and he attributed it to overwork and exposure to cold and wet.

April 14th. Ordered beef-tea, and a mixture containing small doses of antimony.—15th: Cough less frequent; no expectoration. Had been delirious last night.—18th: Crepitation is heard, along with sibilus, in both mammary regions. Stop mixture.—19th: Crepitation audible over both backs. Cough has almost ceased, and there is still no expectoration. Nocturnal delirium continues. Pulse 120, weak. He is unable to stand when taken up to stool, and has occasional rigors followed by profuse perspirations. To have port wine and an extra allowance of beef-tea.—20th: Cough has entirely ceased. Left side of chest expands more than right. Breathing still forced; is now more abdominal. Dulness on percussion in right infra-axillary region. Crepitation and sibilus over other parts of chest. Great thirst. Tongue dry, with thick brown fur; dental sordes. Bowels regular. No abdominal tenderness on pressure, and no rash on skin.—21st: Confused and delirious to-day. Brandy to be added to the wine.—22nd: Diurnal

wandering and nocturnal delirium continue.—23rd: Bathed in profuse perspiration; pulse 130, fluttering; is evidently sinking. Died at 4 p.m.

Autopsy, nineteen hours after death.—(Reported by Dr. Beveridge, Pathologist.)—Between one and two pints of sero-sanguinolent fluid was found in the right pleural cavity. The right lung weighed 34 oz., and the left 29 oz.; both were in a state of venous engorgement, and equally and thickly studded with grey tuberculous granulations. The tubercles in the apex of each lung were about the size of split peas, and were larger, softer, and more isolated than those in the base, where they were of the size of pins' heads. The pericardium contained about half an ounce of fluid. The heart was small and flabby. The liver was very soft, and had small yellow tubercles sparsely scattered through its substance. The kidneys, small and softer than natural, were thickly studded with grey tubercles. Intestines very pale; mucous membrane not examined. Spleen healthy. Brain not examined.

Remarks.—The duration of the disease in this case was less than four weeks. After the examination on April 14th I set down the case as one of capillary bronchitis; but for the next four days I could not reconcile in my own mind the intensity of the febrile symptoms and the dyspnoea with the comparatively slight amount of bronchial râles, the diminution of the cough, and the absence of expectoration; and when I found on the 19th, that the nocturnal delirium was increasing, prostration rapidly advancing, and sordes forming on the teeth, I was disposed to look upon it as a case of typhoid fever, although there were no rose-coloured spots, abdominal pain, or diarrhoea. There was none of the orthopnoea usually found in capillary bronchitis, and during the whole period of the illness he lay almost entirely on his back with the shoulders moderately raised.

Case 2.—J. S., aged twenty-two, a farm servant, was admitted into the Royal Infirmary on Jan. 7th, 1871. About five weeks previous to his admission he got wet, and a few days after he had, as he supposed, a severe cold, which, however, did not disable him from work for nearly two weeks. About the middle of December the cough became very severe, and the expectoration was scanty. His appetite was bad and thirst great.

On admission he complained of weakness, want of breath, and pain in the chest, while his general expression was dull and stupid, and his countenance of a dusky red, almost approaching to lividity. Inspection showed that the chest movements, even on deep inspiration, were very restricted. The percussion note over both sides was equal, and, although not very resonant, still could not be called dull. On auscultation nothing was to be heard beyond soft feeble breathing. The number of respi-

rations was 40 per minute. The sputum was scanty, frothy, and viscid. The urine contained no albumen, and the bowels were regular. The temperature was 102.2° at noon. The next two days he felt easier and better, and he was ordered beef-juice and four ounces of wine. On the 11th he complained of diarrhoea, but it was slight; and sub-crepitant râles were heard over the right lung. On the 14th he was delirious. On the 16th his temperature was 104° at noon. He died on the 17th.

At the autopsy the lungs were found much congested and equally disseminated with miliary tubercle, which in the apex of the left lung was beginning to break down into a few cavities about the size of split peas. The right lung weighed 49 oz. and the left 55 oz.; the liver 60 oz.; the right kidney 5 oz., the left $4\frac{1}{2}$ oz., and they each contained a few tubercles about the size of pins' heads. The spleen weighed 8 oz., and was profusely studded with very minute tubercles about the size of pin-points. The mesenteric glands were healthy, but the solitary glands of the large intestine were enlarged and a few of them ulcerated. The small intestine was congested, but Peyer's patches were healthy.

Remarks.—Although the duration of the disease in this case was longer than in that of W. G., still it was not more than seven weeks from beginning to end. I was struck at the very outset by the appearance of the man's face, which indicated very plainly that there was an impediment to the passage of blood through the lungs, and to its proper oxygenation. The general febrile condition, delirium, and diarrhoea, gave a certain resemblance to a case of enteric fever; but the history of cough and pain in the chest, the dyspnoea and lividity of face, with the almost negative information afforded by the physical examination, led me to conclude that the case was one of acute tuberculosis. In fact, a livid countenance, dyspnoea, and greatly quickened respirations, with feeble breath-sounds, or comparatively few rhonchi, and a high temperature, together with the absence of dulness on percussion and of bronchial breathing, seem to be the most positive signs of the existence of a *primary* pulmonary tuberculosis.

It may be as well to state here that I hold with Niemeyer that, "in the present stage of science, there is but one kind of tubercle—miliary tubercle, and but one form of tuberculosis—miliary tuberculosis."

Niemeyer indicates in his lectures on Phthisis that the temperature-curves in that disease are almost as regular as in typhoid fever or acute pneumonia. I cannot, however, find, either in his book or in Wunderlich's (translated by the Sydenham Society), any very distinct guide as to the type of temperature in acute tuberculosis, nor do I see how there can be much uniformity. The extent and type of the tuberculosis seem to

modify the temperature very considerably. So much is this the case that Wunderlich states that when tubercles occur in patients suffering from advanced phthisis, pneumonia, or cerebral disease, they sometimes fail to affect the temperature at all, or that at least their influence is very slight. Dr. Woodman, his translator, also expresses his conviction that in some cases miliary tuberculosis does not affect the temperature at all. This opinion is certainly opposed to the statement of Niemeyer, and to the experience of Dr. Sidney Ringer as stated in his book on the Temperature in Phthisis, and to my own so far as my limited observation of the temperature in these cases goes. I can only barely imagine the absence of any rise of temperature as occurring in cases of localised secondary tuberculosis with scanty deposit. The general conclusion of most observers seems to be, that, in primary tuberculosis, the more acute the pyrexia, and the more closely it approaches to the remittent type, the greater is its resemblance to enteric fever; but that in tuberculosis the temperature wave is less regular and less high, and the remissions greater. The absence of rose-coloured spots is not to be depended on in the diagnosis, as they are certainly not always present in enteric fever.

In these two cases the absence of cheesy masses in the lungs and other organs tends to disprove the theory, first advanced by Buhl and since accepted by various writers, that acute tuberculosis is a secondary process, the result of infection produced by the absorption of substances which have undergone caseous metamorphosis.

There can be no doubt, from numerous recent observations, that the miliary tubercles are situated in the connective tissue of the lung, in very intimate relation with the blood-vessels, and not in the alveoli. The alveoli may become blocked; but in uncomplicated cases this effect is produced by pressure and the swelling of their walls, and not by exudation (*Rindfleisch*). It is long since Virchow, in his "*Cellular Pathology*," pointed out the lymphoid nature of tubercle, and the very close resemblance of the cells of the tubercle granule with the corpuscles of the lymphatic glands; but it is a question whether the tubercles derive their origin solely from the lymphatic system, as propounded by Klebs, so as to be called the result of a lymphangitis.—*Lancet*, Sept. 26, 1874, p. 444.

27.—ON THE TREATMENT OF PHTHISIS BY THE PHOSPHATE OF LIME AND THE JUICE OF RAW MEAT.

By Dr. HENRY BLANC, Paris.

The juice of raw meat and the phosphate of lime, combined, have given me very good results in severe cases of phthisis.

Both these remedies have been prescribed in phthisis, but at present they are not much in favour. This result is due to their defective mode of administration, as none are more valuable in chronic diseases of the lungs when they are administered in a form suitable to the patient and to the disease.

The assimilation of the phosphate of lime is still an open question. Real progress has been made of late, but, nevertheless, we are far from having attained perfection; and whilst for the present we will do well to avail ourselves of the preparations placed in our hands, yet we must not rest satisfied with them, and trust to time and science to solve this very important question.

In France three preparations of phosphate of lime claim our attention. These are—the lacto-phosphate, the chlorhydro-phosphate, and the monocalcic-phosphate.

Not only science, but, unfortunately, commercial enterprise also has a great interest in this question; and, in consequence, a pamphlet war has broken out between the promoters of some of the soluble phosphates of lime. Dr. Dusart, who patronises the lacto-phosphate, and Mr. Coirre, who prepares the chlorhydro-phosphate, have based their solutions on the supposed nature of the gastric acid; the one, Dr. Dusart, believes that it is the lactic acid, and he recommends the lacto-phosphate of lime; and Mr. Coirre, convinced that it is the muriatic acid, combines this acid with the phosphate of lime.

A third competitor for the favours of the profession, Mr. Barbarin, suggests that it is probable, nay, possible, that both these gentlemen may be right, and he adds, “Why all this discussion? Having to do with a soluble phosphate, of what importance is the gastric juice?” Mr. Barbarin quotes from a recent communication made by Mr. Joulié, the pharmacien de la Maison de Santé, from which it appears that whatever the acid added to the tricalcic-phosphate, the result is one equivalent of monocalcic-phosphate, and two equivalents of another salt of lime; and if the acid employed be the lactic or the muriatic, it forms, with the base, a soluble combination, and we obtain a solution containing a mixture of both salts, and nothing more.

According to Mr. Barbarin, the lacto-phosphate does not exist any more than the chlorhydro-phosphate, and, to use his own expression, “these are two pompous words which only serve to denominate the mixture of one equivalent of monocalcic-phosphate and two equivalents of lactate of lime in the first instance; of one equivalent of monocalcic-phosphate and two equivalents of chloride of calcium in the second.”

Evidently, if this chemical analysis of these salts be correct, then both the chlorhydro-phosphate and the lacto-phosphate will retain, the one under the form of chloride of calcium,

the other under that of lactate of lime, two equivalents of the base more than the quantity necessary to the formation of the monocalcic-phosphate. Such is Mr. Barbarin's conclusion; *ergo*, he recommends the solution of monocalcic-phosphate which he himself prepares.

As long as the question is debated by those only who sell what they laud, the practitioner will be placed under great difficulties, not knowing what to believe or trust. I have prescribed the several preparations above-mentioned, and I have been able to make the following remarks:—The syrup of lacto-phosphate of lime (Dr. Dusart's) is well tolerated at first, but after a while it induces dyspepsia, nausea, and somnolence. Mr. Coirre's solution of chlorhydro-phosphate is not grateful to the patients, but it is well tolerated by those who are able to overcome their dislike to the rough, acid taste of the drug, and it has given me very good results. Another muriatic preparation, the wine and syrup of Chennevière, specially the wine of chlorhydro-phosphate of lime, is a very useful remedy, being both pleasing and well tolerated by the patient. I have only lately tried the monocalcic-phosphate syrup of Mr. Barbarin, not sufficiently to record an opinion as to its value or otherwise.

When the patients have not been able to persist in the use of the above-mentioned preparations of phosphate of lime, I have prescribed instead a drachm to a drachm and a half daily of the syrup of triple phosphates—Easton's formula,—giving at the same time a couple of grains of phosphate of lime at the principal meals.

The cases that are progressing most favourably are those who take the muriatic solutions—Coirre's solution or Chennevière's wine—and at the same time a drachm daily of the syrup of triple phosphates. Under whatever form the phosphate of lime may be prescribed, it should *always* be taken at meals.

The treatment of phthisis by raw meat and alcohol was prominently brought to notice some years ago by Professor Fuster of Montpellier. At first the results published were so favourable that it appeared that consumption had at last been conquered; but, unfortunately, the advantages gained by this treatment were not lasting, not that the raw meat was at fault, but merely because the patient could not be induced after a certain time to swallow the raw meat, or the stomach to tolerate it. The fault rests entirely with the mode of administration, and the principles on which the treatment was based still remain good, and by improving the form under which this valuable remedy can be administered we obtain from it all the advantages possible without any of its inconveniences.

I administer the raw meat in the following manner:—A pound to a pound and a half of fresh beef, deprived of fat, bones, &c., is placed over a quick fire for a few minutes, in order to whiten and harden the external surface only; the piece of meat is then cut into two or three pieces corresponding to the size of the meat-press, and all the juice is extracted by the pressure of the powerful screw. The superficial coction is necessary to overcome the elasticity of the meat, which renders the extraction of the juice a very difficult matter unless more powerful machines be used than the simple one at present required. A pound and a half of good fresh meat gives a teacupful of juice. The juice should be prepared daily. This juice, having all the physical properties of raw meat, is easily digested, is well tolerated, and, served in the following manner, is always very grateful to the patient. The juice should be mixed with equal parts of tepid broth, made of bones and flavoured with salt and pepper, and to which tapioca, vermicelli, &c., can be added. Care, however, should be taken that the broth is never more than *tepid*, otherwise coagulation takes place, and the desired effect is not obtained.

The treatment of the consumptive patient by this method is the following: Early morning: Warm milk (not boiled), with bread and butter, and, if the appetite be good, some fat bacon and eggs. At eleven or twelve o'clock, breakfast, before which a drachm of the syrup of triple phosphate should be taken; during the meal itself, a dose of the muriatic phosphate of lime, and half the daily allowance of the raw meat juice in some broth; the meal should consist, according to appetite and digestive powers, of fish or poultry, or white meats, fresh vegetables, and a few glasses of good alcoholic wine. Dinner at six o'clock on the same principles; broth, with the remainder of the raw meat juice, and, instead of the triple phosphate, a dessert-spoonful of cod-liver oil can be taken with advantage after the meal, if the liver be not enlarged and fatty, and the digestion good. The muriatic solution, or wine of phosphate of lime, should also be taken during the dinner. At night, before retiring to rest, a cupful of warm fresh milk, diluted one-third with Vichy water.

No medicines whatsoever, beyond those mentioned, should be administered, unless some special indications or some urgent symptoms claim their use. Anodynes, narcotics, cough mixtures, lozenges, blisters, inhalations, &c., are practically of no good, and but too often, by lessening the appetite or by irritating the patient, they increase the debility and hasten the fatal end. All hygienic rules—out-of-door exercise, ablution of the skin, &c.—should at the same be carefully attended to. Under the influence of this treatment the appetite rapidly

returns, the cough becomes less troublesome, the expectoration lessens, the night-sweats and all unfavourable symptoms decline and disappear, the patient gains flesh and strength, and the confirmed and helpless invalid, with proper care and prudence, can enjoy life once more.

The last winter in Paris has not been very cold, but it has been marked by variable weather, dampness, and great thermometric variations—all circumstances more unfavourable than cold to persons affected with chronic affections of the lungs; nevertheless all the cases under treatment are doing remarkably well. In all the stethoscopic signs still exist, but in none have they increased, and in several they are less marked.—*Lancet*, June 13, 1874, p. 831.

28.—ON THE USE OF ATROPIA IN PHTHISICAL SWEATING.

By Dr. JAS. M. WILLIAMSON, Resident Medical Officer at the Royal Hospital for Consumption, Ventnor, I.W.

In 1872, Dr. Wilson announced in the Philadelphia Medical Journal that he had successfully treated four cases of phthisical sweating with the sulphate of atropia, and last year Dr. Fräntzel, of Berlin, published an account of a more extended series of researches. Since then, however, little or no attention appears to have been paid to the subject. The following is the result of some experiments which have been made with the drug in sixteen cases under the care of Dr. A. H. Hassall at the Royal National Hospital for Consumption, Ventnor.

The sulphate of atropia is best prescribed in pill, with extract of gentian; watery solutions are not to be depended upon, for they soon spoil by keeping. The first dose should in no instance be larger than one-eightieth of a grain, and guided by the results, the dose may if necessary be increased to one-sixtieth, or even one-fiftieth of a grain; but if this latter quantity be exceeded, well-marked symptoms of poisoning will almost certainly ensue.

In each of the sixteen cases in which the remedy was tried, the first dose produced a distinct effect on the perspiration, either wholly arresting it or materially diminishing it. In only one-fourth of the cases, however, was this effect direct and permanent; that is to say, only four patients, after using the pills for a varying number of nights, were able to omit the remedy without the sweating returning. But in these four the effect was lasting, for when seen two months after the cessation each patient reported that he was still free from the slightest dampness. Of the remaining twelve cases, four found the benefit direct but temporary; that is to say, they obtained complete relief on those nights on which they took the atropia, but the

perspiration returned if the pill was missed. In seven cases, although the eightieth of a grain at first diminished the sweatings, the dose had to be increased to maintain the effect. At last, however, the increasing habituation to the drug which characterised the sweats did not hold good of the toxic symptoms, for these became so marked that the remedy had to be abandoned in all the seven cases, without its having succeeded in putting a stop to the sweats in a single instance. Only one case remains, and here there appears to have been an unusual sensibility to the action of the drug. The eightieth of a grain distinctly lessened the profuse sweating each time it was tried, but after three or four attempts it had to be discontinued on account of the severe symptoms of poisoning to which it gave rise.

It will thus be seen that the remedy controlled the perspirations more or less in the whole of the sixteen cases; that the effect was direct and permanent in four; direct but temporary in four; beneficial but transitory in seven; and that it was inadmissible in only one instance. These results will be all the more striking when it is added that in many of the cases the sweatings had extended over a period of several weeks, and had resisted all the ordinary methods of treatment.

The toxic symptoms most frequently complained of were intense heat and dryness of the throat during the night, and indisposition for bodily or mental effort on the following morning. Vomiting sometimes occurred, but diarrhoea was not observed. In one case there was retention of urine for several hours. The pupil was sluggish in action, but distinct dilatation was not common except in those cases where one-fiftieth of a grain was administered. The patients often complained of dizziness and inability to read any print but that of large type.

It is well known that all efforts to check the night-sweats of phthisis too frequently fail; and although it is not urged that the sulphate of atropia is less uncertain than other remedies, it is believed that it will not be found inferior to them in obstinate cases, in some of which it was of much service after all other remedies had failed. The drug would probably prove of extreme value in combating the perspirations in those diseases (such as acute rheumatism) in which the sweating extends over a comparatively short period, and is not so inveterate as that in phthisis.—*Lancet*, July 25, 1874, p. 116.

29.—ON PARACENTESIS THORACIS.

By JOHN WOOD, Esq., F.R.S., Surgeon to King's College Hospital.

[Some cases lately in the wards of King's College Hospital have given the opportunity of testing the value of the different

surgical ways of treating pleuritic effusions at present in vogue. In one case seventy ounces of thick pus were drawn off with the aspirator, the puncture being made in the usual place below the armpit. A free opening and the use of a drainage tube was however found to be necessary. This case recovered.]

In cases of serous effusion, not purulent, when the dyspnoea imperatively requires the removal of the fluid, the most beneficial results are to be expected from the use of the aspirator.

In using the aspirator, it is of the utmost importance to ascertain and ensure beforehand that the instrument is clean and its joints and taps in perfect order. The pointed tube or needle used for puncturing should always, in the first place, have a stilette pushed through it to clear away any particle of dried and encrusted matter, which may block up the passage, or afford a nidus of putrefaction or infection which may get into the wound and set up their undesirable processes. The india-rubber tube connecting it with the vacuum cylinder must also be washed in carbolised water or solution of the permanganate of potash, to ensure perfect sweetness; and finally, just before using, the needle should be dipped in carbolised oil. Those who are sceptical as to these precautions should remember the results which sometimes spring from a mere puncture with a foul instrument or splinter. Then the syringe itself should be examined to see that no air can be admitted through the joints. If this be the case, the air will show itself in frothy bubbles as the fluid rushes into the vacuum, and there will be a danger of its getting into the pleural cavity at the next stroke of the piston; this might prove to be a cause of putrefactive changes in the pleural effusion. The admission of air is most likely to happen towards the end of the operation, when the fluid is exhausted, or nearly so.

Then as to the choice of the site in performing the operation. In this particular pretty much the same rules apply to the use of Dieulafoy's instrument as to other operations for the same purpose. In all it is advisable to tap the fluid at as low a point as possible, in order to drain it off entirely; for even under the influence of the vacuum suction the canula may clear at first a space around its point, and then the soft and elastic lung expanding to the side of the thorax with its moist and sticky surface, may prevent the lower part of the fluid from rising against gravity into the tube of the syringe. The place chosen in ordinary cases of empyema is usually either under the arm between the fifth and sixth, or with some authorities the sixth and seventh ribs. But this, although just above the level of the diaphragm, at the point where the puncture is made, is much above the floor-level of the hinder part of the pleural

cavity, both as the patient sits up in bed, and much more when in his most usual position, lying down or propped up by pillows. The back part of the diaphragm curves rapidly downwards from the anterior part of the seventh to the vertebral half of the eleventh rib. The serous pleural lining of the thoracic cavity is reflected from the dome of the diaphragm to the eleventh rib, forming a cul de sac, the downward limit of which is the intercostal space between the eleventh and twelfth ribs, or the upper border of the twelfth rib at its centre. The pleura covers the inner surface of the posterior two-thirds of the tenth and eleventh ribs (sometimes, but not always, reaching the twelfth rib), and the whole of the inner surface of the osseous portion and a part of the cartilage of the seventh, eighth, and ninth. The angle at which the diaphragm arises from these ribs and their cartilages, however, is so acute, that but little space is left for the thin lower border of the lung, forming a mere groove for it to work in. This groove is deeper and narrower at the back part, and although when the lung is compressed by effusion this groove is somewhat widened and enlarged, as seen by the bulging of the intercostal spaces at this part, yet it is a matter for accurate manipulation to pass an instrument into the pleural cavity without pushing its point into the sloping surface of the diaphragm beyond. The operator may even, if too bold, transfix the diaphragm and puncture the liver lying close below and moulded upon its concave dome. This is more likely to happen if the liver is enlarged and the puncture happens to be made at the very instant of complete expiration. Sometimes the serous pleural lining is reflected from the diaphragm to the ribs a little higher than usual, and does not come quite as far down as the costal origins of the muscle. Its areolar connexions here, too, are loose, and give way readily to a blunt-pointed instrument, which, if the pleura be also hardened and thickened by disease, may even push this membrane before it, and tear it from its parietal attachments without penetrating the pleural cavity at all; or, as commonly occurs, there may be adhesion of the diaphragmatic to the costal pleura at the lower and back part of the thoracic cavity. As a matter of precaution therefore it is better to choose an intercostal space somewhat higher than the lowest level of the pleural cavity for the first opening into it, although the lowest level may be theoretically the most effective for drawing off the fluid. Again, allowance must be made for any displacements of the heart and liver. If the diagnosis of fluid in the pleura be clear and correct these organs are pushed, the former over to the opposite side, and the latter towards the abdomen somewhat, and thus operative procedures on the affected side are

rendered more easy. But it must not be forgotten that adhesions of the lung either to the diaphragmatic or costal pleura may altogether interfere with this change. Thus, the usually accredited position between the middle of the fifth and sixth or sixth and seventh is the safest place at which to make the first puncture. It is better to puncture the intercostal muscles close to the smooth, convex border of the rib below. Here you are less likely to meet with irregular projections or ridges of the rib. You are also less likely to meet with or injure the intercostal vessels and nerves in this situation. Usually the intercostal artery bifurcates about the middle of the rib, from which point forwards a small branch (scarcely of sufficient importance to need especial care) runs along the upper border of the rib below the space. Posterior to the lateral median line of the thorax the intercostal arteries in the spaces under consideration are of considerable size, and lie above their respective nerves, somewhat protected externally by the outer projecting lip of the intercostal groove, just external to the angle of the rib above. More anteriorly they lie nearer the middle of the intercostal space, and are endangered by an incision or puncture placed too near the upper rib. It must also be borne in mind, in operating in this region, that these vessels may be irregular in their position, or their cutaneous or other branches larger than normal. In the tenth and eleventh intercostal spaces the vessels are usually of somewhat smaller size than the rest, but in operating at a point near the vertebral column the lesser distance of their aortic origin from the seat of operation must be remembered.

But considering that adhesions, more or less extensive, may in any given case exist between the visceral and parietal layers of pleura circumscribing the effusion of matter, and limiting it as in an ordinary abscess, it is better, I think, to be guided in the choice of the site for operation to a great extent by the percussion sounds and stethoscopic indications. If the dulness of the percussion note, and the absence of respiratory sounds be more decided in one place than another, it will be better to choose that part for operating. It is well to be guided also in the choice of a situation by the presence and amount of intercostal bulging, and the absence of the expiratory wave, on principles similar to those which determine the opening of any other abscess as its "pointing" may indicate. And I would hesitate about puncturing even at the most accredited place, unless this sign was in some degree present. Adhesions are indicated mostly by a certain amount of contraction or depression, more visible at the commencement of inspiration at the intercostal space where it occurs.

In all cases an incision, long enough to admit the tip of the

finger, should be made through the skin and fibres of the serratus magnus or latissimus dorsi muscles before the pointed tube or trocar is introduced, otherwise the instrument, especially if slender, may even fail to pierce the skin, or may slip over a rib or be drawn aside by the spasmodic contraction of a muscle, and, slanting off obliquely, may fail to enter the thorax, or may puncture it at a part not exactly contemplated. I make it a point to feel clearly, with the finger, the position of the upper border of the under rib, and in stout subjects also to divide some fibres of the external intercostal muscle before puncturing. In all cases where the puncture is not intended to remain open, the skin should be drawn upwards before using the scalpel, so as to form a valvular covering to the puncture when the instrument is withdrawn. Even where the puncture is a very small one, this increases the safeguards against the admission of air, so important in cases where the effusion is serous only.

In estimating the propriety of tapping the pleura in a case of hydrothorax, where the probabilities are against the presence of pus in the cavity, it should be kept in mind that a spontaneous absorption of the effused fluid as rapidly as it has occurred is a common result, especially in the young. I should say that in such cases tapping ought only to be performed when the symptoms of dyspnoea and evidences of insufficient aeration of the blood are urgent. This is of course more likely to be the case when the effusion occurs on both sides of the chest simultaneously, but this is not common. A real difficulty lies in recognising the character of the effusion before tapping it, and it frequently becomes, in fact, a decision of a question probabilities, which can only be made certain by seeing some of the fluid. In such cases M. Dieulafoy's instrument is a very valuable means of withdrawing and inspecting the fluid without the chance of air entering the chest and setting up a suppurative or putrefactive change. If the case be one of simple hydrothorax, it is by no means necessary to draw off all the fluid to the last drop. Relief of tension and pressure upon the lung are usually sufficient to turn the scale in favour of the absorbents, and to enable them to complete the removal of the fluid which may remain. Over anxiety to remove all the fluid increases the chances of air passing into the vacant cavity.

In cases of empyema, on the other hand, it is important to drain off the pus completely. In children and young persons, otherwise healthy, one complete removal of the pus may be followed by immediate adhesion of the lung to the pleura, and no subsequent deposit may take place. But in adults my own experience tells me that for the most part the successful treat-

ment of empyema becomes ultimately one of free openings, and the establishment of effective drainage and thorough washing out and cleansing with antiseptic fluids. The most mild of these is Condyl's fluid or a solution of the permanganate of potass. The most generally useful and effective is carbolic acid, or some of its preparations in solution; and the most powerful, both as an antiseptic and desiccant of the discharge, as well as by its absorption and general constitutional effect in scrofulous or tubercular cases, is the solution of iodine or compound iodine lotion of the Pharmacopœia. Under its use the discharge is immediately deodorised, and gradually diminished. The lung, if not hopelessly bound down, enlarges to something of its natural volume, the parietes of the chest sink inwards to meet it the rest of the way, and adhesions form around the drainage-tube, converting its track into a sinus which gradually fills up with granulations, and ultimately heals entirely. In employing this remedy the surgeon must keep it in mind its tendency to act on the drainage-tube, and change the latter before it gets so friable as to endanger its breakage during extraction, and, if its condition be doubtful at the time of abstraction, to employ precautions similar to those I have described in the former part of this lecture.—*Lancet*, May 9, 1874, p. 646.

30.—ON TAPPING THE CHEST.

By Dr. T. CLIFFORD ALLBUTT, M.A., Physician to the Leeds General Infirmary, Leeds Dispensary, and Leeds Fever Hospital.

[The difficulties of the diagnosis of pleural effusion are hardly sufficiently recognised.]

Vocal fremitus is the only sign to which we can look for any real help in cases where we have to decide between pleuritic effusion, aneurism, intra-thoracic cancer, or pleural hydatid. Practically, aneurism is usually betrayed by other signs; and hydatid, when it occurs, may be treated as a fluid effusion. But between intra-thoracic growths and effusion the diagnosis is often difficult, sometimes impossible. Suppose, for instance, the growth to start from the root of the lung and to compress the lung from below upwards, and so invade the whole side of the chest; how are we to distinguish it? In such cases vocal fremitus may help us if the bronchial tubes are not wholly occluded; but in the first place they often are, and in the second place vocal fremitus is of no use in feeble persons or in persons whose voices are hoarse or whispering,—and such conditions are common enough. That these difficulties are not

merely speculative my own experience abundantly testifies. Not to speak of pneumonic consolidation, three times at least I have been distinctly wrong in diagnosing pleural effusion when intra-thoracic tumour was present, and many times my doubts have only been removed by the results of exploration. Pleuritic effusion, be it remembered, will occur often enough in middle-aged and cachectic persons, and in them the previous history is of little use. Nor is it so simple as hospital practice would teach, to make explorations. To tap the chest is to the sensitive persons we meet in private practice an "operation," and a "dry tapping" is so far a failure, however good may be the operator's collateral skill in "putting things." To tap the chest requires, of course, certain preparations for the reception of fluid should this be present; and although surgeons may call it painless, I have seen few patients who have borne it without much wincing. In these difficulties Dr. Ringer's use of the hypodermic syringe seems to me to be a very great help. It may not be new to others, but to me it is quite new, and a very happy suggestion. But let it be remembered that one puncture may not decide the matter. Even when fluid is present it may not appear at first, and to give a diagnosis of cancer after one dry puncture might be a sad blunder, as the following case will show:—

In the early part of 1873 I saw the Rev. Mr. M. with Dr. Blythman, of Swinton. His age was sixty-two, and he had been a very vigorous man up to some eight weeks before Dr. Blythman's attendance. Being unaccustomed to care of his health, he had neglected to call in any help, and on Dr. Blythman's first visit the left side of the chest was almost wholly dull. Shortly afterwards we met in consultation, and we had to decide between pleuritic effusion and cancer. Cachexia, loss of flesh, time of life, and some points in family history made cancer not improbable. His voice was loud and firm, and vocal fremitus was absent over the dull region. We pronounced in favour of fluid, and, as tapping was dreaded by the family, we determined upon a course of medicine. I have long taught that the results of the treatment of important pleuritic effusions by medicine are very bad, and I regret that we did not at once overbear the timidity of Mr. M.'s wife and daughters. As the patient lived far away from Leeds, an interval of perhaps three weeks elapsed before we met again; we then arranged to tap the chest, but again some little time elapsed before we met for the purpose. Weiss's newer aspirator was used. The operation was an anxious one, on account of the patient's position in life and of the solicitude of a large family, so that our embarrassment was great when no fluid followed the puncture. Dr. Blythman had inserted the needle in the

axillary line between the seventh and eighth ribs, and he now cleared the needle repeatedly, and altered its depth and inclination, but to no purpose. He then removed the needle, and reinserted it with a sharp plunge, but to no purpose. This was a *mauvais quart d'heure* for both of us, not to mention the patient. We now rapidly reconsidered the history and the symptoms, and determined to operate again. Dr. Blythman accordingly made an incision through the muscles of the back, and inserted the trocar about two inches below the spine of the scapula. To our relief, serum flowed, and about four pints were removed. The heart, which had been much displaced and embarrassed, was relieved, and the patient did well for a time. The chest, however, refilled, and two or three weeks later the patient died suddenly of syncope the day before I had arranged to meet Dr. Blythman again with the view of repeating the operation. Without a good deal of firmness on one part, and of generous confidence on the other, the first puncture would not have been repeated, and the patient would have died under a diagnosis of cancer.

Did space permit I would record a case in which the converse state existed. A boy, aged twelve years, was tapped for me by my colleague, Mr. Jessop, under the unhesitating belief of us both that there was pleuritic effusion. The history was not unlike gradual effusion; it was left-sided, the heart was displaced, the spaces were flattened, there was no vocal fremitus, and the absence of breathing was from below upwards. We tapped unsuccessfully several times, and the post-mortem revealed a large malignant growth springing from the spine. Operation apart, diagnosis in this case was simply impossible. The cases in which diagnosis between pneumonic consolidation and pleuritic effusion proved far from easy are of course more numerous; and if breath-sounds are absent towards the base, as may be the case, and if at the same time vocal fremitus cannot be obtained anywhere in the chest on account of illness or defective voice, then nothing less than Dr. Ringer's syringe will suffice for a decision. But this is unfortunate, for, in private practice, not young ladies only, but others also, will think it strange that a physician cannot make up his mind without plunging an instrument into their chests. I must not be delayed, however, by these considerations any farther than to refer to page 416 of Dr. Ringer's essay (*Practitioner*, Dec., 1873), where he makes mention of difficulties of diagnosis between effusions and some forms of chronic phthisis.

It is to be hoped, therefore, that teachers will beware of speaking too confidently of the ease of distinguishing pleuritic effusions before students, who, at the outset of their own experience, may be dismayed by an unexpected dilemma. When

operation in hydrothorax is required, writers are fairly agreed. It is agreed that when effusions are small they are to be left alone unless a small purulent effusion should disturb the general health. In this case it should be removed at once. Larger effusions, whether serous or purulent, should be removed at once, as they threaten both life and lung. The recorded results of the medicinal treatment of such effusions are very bad; while those of operative treatment are good, or, indeed, excellent. This doctrine has been slowly established, but now is fairly acknowledged by all men of experience. I believe that the dyspnoea (said to be sign of the needed interference) is chiefly cardiac, and in some cases is due to displacement of that organ, and in others to an overloading of the right heart, so that the severe symptoms of a rapidly increasing effusion are very like those of clot in the right heart. The cases in which delay is least excusable are the rapid and degenerating effusions of phthisical constitutions; while on, the other hand, rheumatic effusions often vanish quickly of themselves. This is fortunate, as the commonest kind of pericardial effusion is rheumatic.

I remember being called to assist in at the illness of a young man at Halifax, under the care of Mr. Smith of that town, and Mr. Joseph Teale, formerly of Leeds. He had acute rheumatism, with effusion into both pleuræ and into the pericardium, and his life was in imminent danger. In my own mind I preferred to tap the pericardium, but it seemed scarcely fair to throw the responsibility of carrying out my own plan upon Mr. Smith. So we agreed to tap the left pleura, which was dull up to the spine of the scapula, while the right was dull only up to the angle of that bone. About a pint of rapidly clotting serum was drawn off, and sufficient relief was obtained for the time. Next day the other effusions showed signs of subsidence, and before long had so disappeared that no farther interference was needed. The operation could scarcely have influenced the other two cavities.

The effusions which occur in heart or kidney disease, again do not press for immediate interference, for though they do not readily recede, they generally increase slowly, and do not tend so much to harm the lung. Pyæmic effusions, on the contrary, increase rapidly, and being, of course, purulent, should be removed on the instant of discovery. Too often, however, they will recur as soon as removed. We must operate quickly, then, in rapid serous effusions of non-rheumatic kind and in all purulent effusions. Rheumatic effusions must be watched carefully, and drawn off if they do not recede. Simple dropsies and the effusions of common acute pleurisy may be left for a while, and tapped if they assume any proportions or

remain for some time without diminution. For instance, I should tap any non-rheumatic effusion which fills the chest up to the spine of the scapula, or which displaces the heart; if rheumatic I should watch it for a few days, and tap it if persistent. Smaller effusions, whether dropsical or sub-inflammatory, I should certainly tap if after a fortnight they showed no signs of subsidence.

Should we tap in cases of empyema where the pus has made itself another way? I should say "Yes" in almost every case. Where the opening is through the lung the pus is generally evacuated most imperfectly, the retained quantities are decomposed, and the lung is in great danger of breaking up. When, on the other hand, the opening is through the ribs, such opening is generally sinuous, narrow, and improperly placed. Some of the most satisfactory cures I have to record have taken place in cases of empyemic fistula—pulmonary, intercostal, or both—in which a free artificial opening was the means of rapid and permanent cure. Thirdly, What mode of operation answers best (*a*) for serum? If the serum be merely dropsical, the insertion of a fine trocar anywhere in the lower chest is sufficient: the fluid runs off, the wound is stopped by pad and plaster or a film of collodion styptic, and nothing more is needed. This may be repeated as often as required, for out of numerous cases of this kind I have never seen one which has run into empyema, although no precautions are taken by me against the entrance of air. Acute coagulating effusions in healthy constitutions do not generally recur, and they seldom become purulent, though they may do so, and the entrance of air should therefore be prevented. The rapid sub-inflammatory effusions of weakly persons who are phthisical or of phthisical families, tend quickly to pus whether tapped or not, and the entrance of air or of a dirty trocar hastens the process. For dropsical effusions, then, we require a simple exploring trocar. For inflammatory and sub-inflammatory effusions we require a carbolised trocar or needle, and some apparatus to prevent the entrance of air. One operation, even, in the latter kind, sometimes suffices if the fluid be serous. For instance, I recently attended a weakly man, with Mr. Teale and Mr. Oglesby, whose right chest was very full of serum. One operation sufficed for his cure. The loudly praised instrument called the aspirator may be invaluable in many cases, but in tapping of the pleura I have found it cumbrous and even injurious. The so-called "previous vacuum," I believe to be a dialectic refinement of no practical value, and the issue of the fluid is best regulated by the natural movements of the parts which contain it. For a time I, like others, took up the aspirator with much interest, but only to conclude

that its greatest service is to have introduced into practice a number of very useful trocars and perforated needles. I have now returned to the use of the old trocar with a flexible tube, by means of which we draw off the fluid under water. Nothing can answer better. We may now inquire what mode of operation answers best for (b) purulent effusions? I am glad to notice that Dr. Ringer points out that empyema does not always give rise to hectic. The fact is, the concurrence of hectic depends upon the facilities of absorption. If pus be formed in a fresh pleura, and is moderate in quantity, hectic is sure to follow. If the pus by its increase and pressure closes all the superficial vessels and absorbents of the pleura, then hectic is so far prevented, as absorption is thus prevented. Or, if apart from pressure, the pleura has become spoilt and thickened, so far again are absorption and consequent hectic unlikely. Hectic or no hectic, however, it is now, I think; universally agreed that the pus must come out irrespective of its quantity. But opinions differ as to the mode of its removal. Numerous cases of my own, both in and out of hospital, have led me to the strong opinion that a free opening should be made at once. I cannot call to mind a single case, in which closed operations succeeded in effecting a cure, and in all my own cases I believe we have had to come to an open operation sooner or later. Nay, more, I am convinced that in three cases the closed operation has done harm. By relieving the intra-thoracic pressure, absorption has become possible, and hectic has set in. The open operation, on the contrary, if so managed as to give a free outlet to the pus, always allays and generally removes hectic. At the same time, I have to allow that Dr. Ringer has some cases which tell in favour of a closed operation, and as the best kind of open operation is very painful, I shall certainly test this point over again. Of late, when once satisfied that pus exists, I have regarded all closed operations as at best a waste of time. Since the introduction of the aspirator I have repeated my trials of the closed operation with no better results. To turn now to the open operation, and the varieties of it. There is no doubt in my own mind that the posterior operation, recommended by Dr. Bowditch in his letter to me, which was published in the Practitioner (April 1873), gives by far the best results. It will be remembered that Dr. Bowditch, after gauging the depth of the full pleura by the line of lowest resonance over the empty one, enters the cavity a little above this line, by dividing the muscles obliquely and passing by a free opening through the pleura in the line of the angle of the scapula. I have now tried this operation in eight cases, with the best possible results. In two of these, repeated aspirations had ended in perforation of the lung with

escape of pus upwards. Both patients were in extreme illness. By the posterior operation and confinement of the patients to bed, very satisfactory improvement took place. In another case traumatic pleurisy was followed by empyema. An opening had been made between the sixth and seventh ribs in the mammary line before I saw the man, but in spite of this he languished and burnt away at an average temperature of 101° . When he came under my care, I closed the front opening and made the posterior incision. The man very rapidly recovered, and remains in perfect health. The history of a third case illustrates the difficulties which may meet us in tapping the chest. A young and otherwise healthy man came under my care at the infirmary with a filled left pleura and displaced heart. There was no doubt of the diagnosis, though oddly enough he lay throughout his illness on the sound (right) side. I ordered him to be tapped at once, and this was done with the aspirator. Eighteen ounces only were drawn off—to my great disappointment, for I estimated that at least five pints were present. On my next visit I ordered the puncture to be repeated in another place. Again but four ounces were drawn off. Curiously enough this seemed to be, and no doubt was, followed by some absorption and subsidence of the fluid, and we hoped that no farther interference would be needed. We were wrong, however, and the chest again filled. I now determined to be present at the operation myself, and to ask my surgical colleague Mr. Jessop to operate. He kindly used for me Robert's trocar, and again changed the place of puncture; but again no more than twenty ounces could be drawn off. This time the fluid was tinged with pus, and so I determined to make a free posterior opening. Five days later Mr. Jessop did this, and let out five pints at least of curdy pus. The patient is now doing well, though I regret to say that a fistulous opening formed through his lung the day before the operation. The advantage of the free posterior operation is the perfect drainage, to which both dressers and nurses freely testify. The patient must be kept in bed; no pus can putrefy in the thorax; and no injections will be required in uncomplicated cases. The patient must be rigorously confined to bed, not only for the position's sake, but also to avoid chill. Chill is the great cause of relapse in open pleuritic abscess, and a sudden chill may undo in a day the gradual gain of a fortnight. The drawbacks to the posterior operation are—1. Its liability to close; 2. Its severity. The liability to close is due, of course, to the depth of the wound, but with common care and good drainage-tubes we easily overcome this difficulty. The second objection is a grave one. An incision through the deep tissues of the back of the chest is nowadays a serious

operation to perform without an anæsthetic. Now, this objection again is readily obviated if we may give anæsthetics. May we do so? My own feeling is strongly against anæsthesia in a person whose respiratory margin is so small and whose heart is probably displaced. But experience alone can decide this. In one only of my cases was chloroform given, and the result was fairly good. Dr. Bowditch, however, has been less fortunate. In a letter which I lately received from him he says of three cases in which ether was given—"The first, a female, bore it with some difficulty; the pulse fluttered and there was some lividity. She soon recovered when the sponge was removed." This woman went on well for a fortnight, and then died suddenly of purulent effusion into the pericardium, which was not suspected during life. In the second case, also a young woman, "death seemed so imminent during the operation, from the effects of the ether, that the most energetic means were resorted to. Tracheotomy and artificial respiration were employed, and the patient recovered at the time." But she never overcame the harm of the anæsthesia, and died in a few days (of exhaustion?) In the third case, the "woman died almost immediately after the operation, and it was admitted that the anæsthetic, to say the least, hastened, if it did not in that particular case cause death." Dr. Bowditch then refers me to four cases, published by Dr. John G. Blake, of Boston, all of which resulted favourably, but in none of them was any anæsthetic used. In a fifth and subsequent case, however, Dr. Blake used ether. "Immediately after recovery from the ether effects a severe rigor took place. This was repeated three days later, and death followed from debility and prostration." No autopsy was allowed. "Finally (says Dr. Bowditch) Dr. Firfield informs me that in one case in which he gave ether he was obliged to forbear, and finish the operation without perfect anæsthesia, the symptoms attending it being of so threatening a character that he dared not to press it." These gentlemen are agreed that the use of anæsthesia when one lung is wholly useless is to be avoided. The outcome of this is, in Dr. Bowditch's opinion, that aspiration should be used in empyema at first, the operation being stayed on the appearance of any signs of discomfort; that if a further operation be needed it must be done by free incision, without anæsthesia; that if this be refused a large trocar must be inserted, with subsequent drainage-tube of spiral wire or indiarubber, all, of course, without anæsthetic.

I differ from my valued correspondent as follows:—I have found that aspiration rarely brings about a cure, though no doubt my experience may need correction on this point; that it is certainly far from painless; that the relief of pressure

without free issue for the pus not uncommonly gives rise to absorption and hectic; and, finally, that in three cases treated by aspiration I did not find that this relief stayed the passage of pus outwards by the lung, so that pulmonary evacuation occurred before incision was practised. I hold, therefore, that free incision at the earliest time is far best for the patient, and that against this lies only the pain of it, concerning which the physician must use his tact in each case. Perhaps local anæsthesia may help us, or other general anæsthetics may be less dangerous than ether. In my one case, chloroform proved harmless. If it be urged that ulceration through the lung or septic absorption are avoided by very frequent aspiration, I should reply that repeated aspirations are certainly more distressing than one incision, and that patients of mine have always shrunk from repeated aspirations. Careful reports of a score or two of aspirated cases will no doubt do much to settle the matter, and for such we must await.

In conclusion, I may turn to the details of physical signs after operation, in order to say that those who expect to find a decrease of morbid signs in proportion to the decrease of the fluid contents of the pleura will nearly always be disappointed. We have to judge of the ebb and reflux of the serum or pus as well as we can from all the circumstances of the case. An expanding lung will force a small remnant of fluid over a large surface; and it is well known, no doubt, to practised observers that great tracts of dulness will remain long after all fluid of importance is removed, and, indeed, may never wholly disappear.—*Medical Times and Gazette*, May 9 and 26, 1874, pp. 497, 526.

31.—ON DISEASE OF THE ŒSOPHAGUS; WITH SPECIAL REFERENCE TO ŒSOPHAGEAL AUSCULTATION.

By Dr. MORELL MACKENZIE, lately Physician to the London Hospital.

[We are indebted to Dr. Hamburger, an Austrian physician, for establishing the method of examining the Œsophagus by auscultation.]

Auscultation of the Œsophagus is very easy, but it requires considerable practice and much patience; practice, because it is requisite to get the ear well accustomed to the normal Œsophageal sounds; patience, because in each case it is necessary to apply the stethoscope successively down the whole length of the Œsophagus, and to listen attentively at each spot. Let me explain to you Hamburger's method, as applied to a healthy man. We direct the patient to take a mouthful of drink—water does

very well for the purpose, but a thickened fluid, such as gruel or arrow-root, answers better. We then apply the stethoscope, make a sign to the patient to swallow, and listen attentively: as the small portion of fluid, or if you will allow me somewhat arbitrarily to call it "the morsel," passes down the throat it produces various sounds, and conveys certain impressions to the mind of the auditor; the proper interpretation of these sounds constitutes the art of œsophageal auscultation. If the stethoscope be applied on a level with the hyoid bone on the side of the neck, and the patient be directed to swallow a morsel, a loud, gurgling noise is heard, which may be called the "pharyngeal sound." The word "glou-glou" has been said to imitate the pharyngeal sound, and occasionally a sound which may be represented by this word is undoubtedly heard. In order to get an idea of it "glou-glou" should be pronounced in a loud whisper; but it must be understood that in the greater number of cases the sound bears no resemblance to this word. If instead of listening in the neck the stethoscope be applied to the left side of one of the dorsal vertebræ, the true "œsophageal sound" becomes audible. The pharyngeal sound, which is due to the sudden passage of air and liquid into the pharyngeal cavity, is sometimes so loud, and so distinctly conveyed down the œsophagus, that it obscures the true œsophageal sound. In these cases it is better to let the patient take a continuous draught of water. By this means the intermingling of air and water is greatly diminished, and the œsophageal sound may be detected. The sound which is heard conveys the idea of the rapid passing downwards of a small spindle-shaped body of fluid consistence; the sound is sharp and sudden, and ceases immediately. Hamburger describes the sound as being characteristic of an egg-shaped body, about an inch in length, and half an inch in breadth, the small end of the egg being above and the large end below. Hamburger also thinks that the shape of the morsel affords a strong indication as to the condition of the muscular walls of the œsophagus, the lower end of the morsel or egg-shaped body being blunted or truncated in proportion to the feebleness of the muscular action. I must confess, however, these are refinements that I have not been able to arrive at.

The principal points which we have to consider are—first, the character of the œsophageal sound; and, secondly, the quickness of the act of deglutition. In some cases we shall find that the sound is very feeble, and occasionally altogether absent; sometimes we hear (and this often happens in organic stricture) a confused and continuous bubbling sound, lasting for several seconds; sometimes the sound is accompanied with a grating noise. The quickness of the act of deglutition is also of some

importance, and is determined by placing the hand on the hyoid bone whilst the practitioner auscultates the œsophagus posteriorly: as the patient commences to swallow, the operator feels the hyoid bone rise, and can thus estimate the length of time which elapses before the morsel reaches that portion of the œsophagus which is being auscultated. The rapidity of the act varies in different people in a state of health, and can always be made to take place quite slowly. This will be at once apparent by directing a healthy man to keep on swallowing for a few minutes some rather difficult substance, such as a mealy potato. Under ordinary circumstances, the lapse of time between the entrance of the morsel into the œsophagus and its arrival opposite the stethoscope placed at the side of the eighth dorsal vertebra is so short that it cannot be determined; but after swallowing several mouthfuls of potato without drink, two or three seconds elapse before the morsel arrives at the lowest part of the œsophagus.

Information can not only be obtained as to the passage downwards of the morsel, but its regurgitation can also be perceived when from any cause it cannot descend into the stomach. And the mode in which this regurgitation takes place sometimes enables us to distinguish between a spasmodic and an organic stricture; for whilst in the latter case an appreciable time elapses before the food is forced upwards, in spasmodic stricture the regurgitation is instantaneous. Hamburger also attaches importance to the direction which the morsel takes, and asserts that in some cases, where the œsophagus is pressed upon by a tumour in the posterior mediastinal region, the œsophageal sound may be heard more distinctly on the right side of the vertebræ than on the left; and, further, that in cases of disease of the vertebræ the œsophageal sound becomes deadened and indistinct.

Difficulty of swallowing may depend on a general awkwardness in the whole act of deglutition, or on a distinctly localised trouble. Deglutition being one of those compound acts which, though voluntary in its early stages, afterwards becomes involuntary, it is natural that it should be greatly influenced by anything affecting volition. Hence it happens that the mere idea of not being able to swallow often produces such a paralyzing influence that the inception of the act is disturbed. It is therefore obvious that we have to deal with very complex phenomena, and any addition to our means of diagnosis must be accepted with gratitude.

Valuable, however, as are the various special modes of examination, it is most important that you should pay particular attention to the general condition of the patient. Above all things in œsophageal cases you must notice the general

state of nutrition. As diseases of the œsophagus always give rise to dysphagia, and dysphagia leads to a smaller amount of food being taken, it is obvious that in any serious disease emaciation must exist. If, therefore, a patient complain of long-standing dysphagia, and yet present no sign of losing flesh, you may be sure that there is no organic disease. In practice you will occasionally meet with stout, well-nourished women who declare that they cannot swallow at all. You will at once recognise that the obstruction in the œsophagus cannot be of much consequence. Again, you will recollect that organic disease of the œsophagus is extremely rare under forty years of age, and that its most common period of development is between fifty and sixty. It is well to bear in mind also that syphilitic disease of the œsophagus is far more common in men than in women. It is rarely met with under thirty, most commonly it is found between forty and fifty, rarely after the latter age. Nervous affections of the œsophagus, of course, most commonly affect the female sex; they are frequently met with in young delicate girls of the upper classes, but women of all ages and social conditions suffer. The men who are affected are always those of highly emotional temperament. Simple loss of power is, as might be expected, most commonly met with in old people. Here there is often a difficulty in making out a differential diagnosis between impaired innervation and malignant disease, but the absence of expectoration in the former cases is the determining point. With these few preliminary observations I will pass on to the patients themselves.

I will first bring under your notice the case of J. P., aged sixty-one, who was admitted into the hospital June 1st, 1873, suffering from dysphagia of seven weeks' duration. The disease had commenced in the previous February, and had been gradually getting worse, and when admitted he was only able to swallow fluids. He stated that he had twice had similar attacks—one in 1862, the other in 1869, but he acknowledged that he had suffered from constitutional syphilis in 1855. When first seen he was feeble and badly nourished. There was no evidence of lung disease, and he had neither cough nor expectoration. On examining his throat, the pharynx was seen to be healthy, with the exception of a slight cicatricial puckering of the anterior pillar of the fauces on the left side. The larynx was healthy, and there was no obstruction at the orifice of the œsophagus. On directing the patient to drink and auscultating the œsophagus, it was perceived that the morsel, on reaching a spot opposite the sixth dorsal vertebra, was thrown upwards or regurgitated with a loud splashing noise; the regurgitating fluid did not, however, come back into the mouth, but, after an interval of four or five seconds, passed down a second time be-

yond the point of obstruction, and reached the stomach. After repeating this experiment several times the morsel was regurgitated to such an extent that some of it passed into the larynx, and produced a violent attack of coughing. On attempting to pass a No. 8 bougie, a tight stricture was perceived at the junction of the lower third with the upper two-thirds of the œsophagus, and the introduction of the instrument caused slight bleeding.

The case was diagnosed to be one of syphilitic inflammation or ulceration of the mucous membrane of the œsophagus, and the patient was treated with iodide of potassium, ice, and guimauve lozenges. At the end of ten days he was able to swallow without apparent difficulty, although on auscultation it was perceived that the morsel travelled very slowly to the lower part of the œsophagus.

On examining the patient now, you will find that there is not only a certain slowness in the act of deglutition, but that there is a distinctly rough and grating noise, or friction-sound, as the morsel passes the sixth dorsal vertebra. There can be little doubt that in this case the affection was a syphilitic ulceration of the œsophagus, and that it has left some roughness at its former seat. Although this case has progressed extremely favourably, it is important to bear in mind that further thickening is likely to take place. It must be understood that the case is relieved, not cured, and that any error in diet or excess in alcohol is likely to irritate the weak spot, and lead to a recurrence of the stricture.

I may mention that, although these cases of syphilitic ulceration of the œsophagus often yield to iodide of potassium, they are occasionally followed by the growth of a truly malignant formation; it is very important therefore always to give a very guarded prognosis.

The next case which I have to bring before you is an exceedingly interesting one, as it illustrates malignant disease at a very early stage, and particularly exemplifies the value of Hamburger's method. It will also serve to call your attention to the occasional implication of a recurrent nerve in a carcinomatous growth in the immediate neighbourhood of the œsophagus.

S. C., aged forty-seven, was admitted into the hospital on the 13th of May, 1873, on account of difficulty of swallowing, which had been coming on for six weeks. He is a tall man, 5 ft. 11 in. in height; but much emaciated, of cachectic appearance, and when admitted weighed only 9 st. 1 lb., although he stated that his weight six months previously had been 12 st. 4 lb. He said that he had never had syphilis: and there were no traces of the previous occurrence of that disease in the pharynx,

larynx, or any other organ. He stated that he could generally swallow liquids, but that they sometimes went "the wrong way," and gave rise to violent attacks of coughing. Solids, in the ordinary sense of the term, will not pass down at all; but he can manage bread and milk with comparative ease. On examining him with a stethoscope, we perceive a distinct hitch opposite the third dorsal vertebra, and notice that the whole act of deglutition is performed very slowly. It is impossible either above or below the seat of stricture to appreciate anything at all characteristic of a spindle or egg-shaped body, and all attempts to pass a bougie altogether fail. On examining him with the laryngoscope, the right vocal cord is seen to be immovably fixed midway between adduction and abduction; but the larynx is otherwise healthy. His breathing is not perceptibly impaired; but his voice is shrill. His condition has altered very little since his admission, with the exception that he now expectorates in considerable quantities. He has, however, gained 5 lb. in weight since he became an inmate of the hospital.

The questions which now arise are: First, What is the nature of the narrowing of the œsophagus? Secondly, How is it that, if the patient be able to swallow liquids, and even semi-solid substances, a bougie cannot be passed through the stricture? I unhesitatingly reply to the first question that the disease is malignant, and I ground my diagnosis on the fact of the implication of the recurrent laryngeal nerve. I have never known any intrinsic disease of the œsophagus, except cancer, to be associated with pressure on the recurrent nerve. Of course we often meet with cases of extrinsic narrowing of the œsophagus dependent upon mediastinal tumours where the recurrent nerve is involved, but in this case there is no question of an external tumour. Syphilitic disease of the œsophagus never gives rise to pressure on either of the recurrent nerves. With regard to the second question, the impossibility of passing a bougie is no doubt to be explained by the occurrence of a certain amount of spasm when an instrument is introduced. In giving a prognosis as to the probable duration of life in this case, if we were guided by the bougie alone, we should probably imagine that the patient could not survive beyond a few weeks; but with the information obtained by the stethoscope we may reasonably conjecture that he may live for some months. The increased expectoration is probably due to dilatation of the gullet above the seat of stricture—not, as you perhaps might imagine, to ulceration of the surface of the œsophagus. The continued obstruction at the seat of stricture has led to slight dilatation, and this dilatation has produced a pouch-like cavity. In the healthy condition a certain quantity of

saliva is being constantly unconsciously swallowed ; but in this poor man's case the mucus collects above the seat of stricture, and when a certain accumulation has taken place it begins to be expectorated. The implication of the recurrent nerve not only assists in diagnosis, but it also effects prognosis, for in itself it increases the dysphagia, and thus aggravates the starvation which slowly leads to death. In the ordinary act of deglutition, not only does the epiglottis cover the larynx, but there is every reason to believe that the ventricular bands and vocal cords become respectively approximated. In examining a patient with the laryngoscope and directing him to swallow, we do not see the complete approximation of these various parts, because the epiglottis in descending quickly occludes them from view ; but we can perceive that, as the epiglottis descends, the ventricular bands and vocal cords approach each other. We know also that paralysis of the vocal cords often produces dysphagia, even when there is no disease (either intrinsic or extrinsic) of the œsophagus.

The next case which I shall bring under your notice is that of Mary R., an unmarried woman, aged fifty-four. She had enjoyed good health, and led an active and laborious life, up to two years ago, when she began to suffer from severe indigestion. A few months later difficulty of swallowing commenced, and she has now great trouble in taking either solids or liquids. On directing the patient to swallow, you will perceive that deglutition is accompanied with a loud gurgling noise, which can be heard at a distance of three or four yards. On auscultating her in the upper dorsal region this sound is heard much more distinctly ; but below the fifth dorsal vertebra there is a peculiar tympanitic sound, such as might be expected to occur from the passage of a small quantity of fluid into a gaseous chamber. And this is what no doubt actually occurs ; the lower part of the œsophagus and the stomach are distended with gas, and it is this gas which really causes the obstruction to the passage of the morsel. The cause of the dysphagia is to be found in imperfect digestion. These cases are comparatively rare. They generally yield to careful treatment of a therapeutic and dietetic character. The patient should be directed to take fluids in great moderation, and they should only be drunk at the end of a meal ; charcoal or carbolic-acid lozenges are often of great service. On the other hand, it is important to avoid all local treatment of the œsophagus. Bougies and other instruments only irritate the œsophagus, and lead to dilatation of that tube.

The last patient that I shall bring before you to-day is a man whom I have treated at intervals for the last eight years, and this circumstance will at once assure you that there is no organic disease present.

W. E., aged thirty-two, a delicate-looking, poorly-nourished man, first became affected with difficulty of swallowing eleven years ago, the symptoms having first shown themselves after an attack of fever. When seen by me eight years ago he was rather worse than he is now, but he has undergone many changes in the meantime, sometimes having been scarcely able to swallow at all, at other times having been almost well. If you examine him now with the stethoscope you will notice a remarkable slowness in the act of deglutition, and a complete absence of the true œsophageal sound. His case is one of partial paralysis of the constrictors of the œsophagus, and of the muscular coat of that tube; the muscles no longer grasp the bolus, and it can therefore only descend by simple gravitation. He therefore swallows very slowly, and he will tell you that to eat his dinner comfortably he requires two hours, or at least one hour and three-quarters. This, though a moderate time for the consumption of a *diner à la Russe*, does not suit an ordinary mechanic, so he is obliged to eat a small quantity of food very frequently. Faradisation applied directly to the œsophagus is the only remedy that does him any good; it does not cure him, but it immensely improves his deglutition. Sometimes he is able to dispense with treatment for several months, and he once remained cured for two years, but he generally requires faradising every two or three weeks. The patient is a lapidary, and at one time suffered from "lapidary's cramp" with paralysis of several muscles of the thumb and index-finger of the right hand, especially of the abductor, adductor, and short flexor of the thumb, and of the abductor indicis. The nutrition of these muscles was thoroughly re-established by faradisation, and he has never had any recurrence of the disease. The weakness of the muscular structure of the œsophagus and of its constrictor muscles cannot, however, be overcome, probably because the faradaic current cannot be so exactly or so continuously applied as to the muscles of the hand. In these cases it is important to encourage the patient in taking food to swallow a large bolus. The smaller the morsel the greater the difficulty in exciting the contraction of the œsophagus.

I have reserved my observations on the subject of diet in diseases of the œsophagus until now, although that is a very important matter. All food must be very nutritious and non-irritating. Alcoholic stimulants are injurious when there is organic disease, whether malignant or inflammatory. On the other hand, they are very beneficial in cases of functional weakness—especially if taken at the commencement of a meal. Fluids alone should be used where there is organic disease; solids or semisolids in the neurotic cases. Freshly cooked meat

converted into a paste, or *panada*, by a masticator or sausage machine, and served with gravy, can sometimes be taken even better than fluids. Milk, however, always remains the staple diet, and the patient should be encouraged to take several pints a day. In all cases of dysphagia it is very important to attend to the state of the teeth. I have known many cases of difficulty of swallowing, especially in old people, cured by a set of artificial teeth; and even in cases of organic disease, when the treatment can, at the best, be only palliative, improved mastication may prevent the troublesome *contretemps* of the sudden arrest of a foreign body in the œsophagus.—*Lancet*, May 30, 1874, p. 753.

32.—ON IPECACUANHA SPRAY IN WINTER COUGH AND BRONCHITIC ASTHMA.

By Dr. SYDNEY RINGER and WILLIAM MURRELL.

[The writers were first induced to try the effect of inhalation of ipecacuanha spray, by the successful use of a secret remedy by a well-known practitioner. The observations were made during January and February. No medicine was given.]

We shall first refer to winter cough. We have made observations on twenty-five patients, whose ages varied between forty-five and seventy-two, with one exception—that of a woman of thirty-two years. We purposely chose severe cases. In order to avoid burdening this paper with too much detail, we give here a typical case, which will serve in most points to illustrate the condition of the patients. Subsequently we shall report two actual cases in full.

The patient has been troubled with winter cough perhaps for many years. During the summer he is pretty well; but during the cold months, from October to May, he suffers sometimes without intermission, occasionally getting a little better and then catching cold; or perhaps he may lose his cough for a few weeks, but again takes cold on the slightest exposure. So short is the breathing that he can walk only a few yards, especially in the cold air, and finds it hard work to get upstairs, and is often quite unfitted for active life. The breathing grows worse at night, so that he cannot sleep unless the head is propped up with several pillows. He is troubled, too, with paroxysmal dyspnoea, usually at night, which may last several hours, and compels him to sit up. Sometimes the breathing is difficult only on exertion; and in those cases it is made much worse by fogs, east winds, or damp. The expectoration varies greatly; in a few cases there is very little; usually, however, it is rather abundant, and consists of mucus or pus, often with

little or no rhonchus in the chest. It is often difficult to expel the expectoration. The cough is generally very violent, frequent, hacking, and paroxysmal, and the fits may last ten or twenty minutes, and even excite vomiting. They are generally brought on by exertion; nay, in bad cases so easily are they provoked that the patient is afraid to move, or even to speak. The cough and expectoration are much worse in the morning on waking. Sometimes the cough is slight, and then the expectoration is generally scanty, the distressed breathing being the chief symptom. The patient generally wheezes badly, especially at night, and in a bad case the legs are swollen. The patient is emphysematous; there is often no rhonchus, or only sonorous and sibilant or a little bubbling rhonchus at both bases.

In this common but obstinate complaint our results have been very striking, although in many of our patients so bad was the breathing that, on being shown into the outpatients' room, they dropped into a chair, and for a minute or so were unable to speak, or only in monosyllables, having no breath for a long sentence. We used the ordinary spray-producer, with ipecacuanha wine pure or variously diluted. On the first application it sometimes excites a paroxysm of coughing, which generally soon subsides, but if it continues a weaker solution should be used. The patient soon becomes accustomed to it, and inhales the spray freely into the lungs. At first a patient inhales less adroitly than he learns to do afterwards, as he is apt to arch his tongue so that it touches the soft palate, and consequently less enters the chest than when the tongue is depressed. The spray may produce dryness or roughness of the throat, with a raw sore sensation beneath the sternum, and sometimes it causes hoarseness; whilst, on the contrary, some hoarse patients recover voice with the first inhalation. As they go on with the inhalation, they feel it getting lower and lower into the chest till many say they can feel it as low as the ensiform cartilage.

The dyspnoea is the first symptom relieved. The night after the first application the paroxysmal dyspnoea was often improved, and the patient had a good night's rest, although four months before the sleep was much broken by shortness of breath and coughing. The difficulty of breathing on exertion is also quickly relieved; for often after the first administration the patient walked home much easier than he came to the hospital, and this improvement is continuous, so that in one or two days or a week the patient can walk with very little distress, a marked improvement taking place immediately after each inhalation; and although after some hours the breathing may again grow a little worse, yet some permanent improve-

ment is gained, unless the patient catches a fresh cold. We have heard patients say that in a week's time they could walk two miles with less distress of breathing than they could walk a hundred yards before the spray was employed. In some instances two or three days' daily spraying is required before any noticeable improvement takes place, this comparatively slow effect being sometimes due to awkward inhalation, so that but little ipecacuanha passes into the bronchial tubes. The effect on the cough and expectoration is also very marked, these both greatly decreasing in a few days, though the improvement in these respects is rather slower than in the case of the breathing. Sometimes for the first few days the expectoration is rather increased. It speedily alters in character, so that it is expelled much more readily, and thus the cough becomes easier, even before the expectoration diminishes.

Treated in this way the patient is soon enabled to lie down at night with his head lower, and in a week or ten days, and sometimes earlier, can do with only one pillow. This improvement occurs in spite of fogs, damp, or east winds—nay, even whilst the weather gets daily worse, and when the patient is exposed to it the chief part of the day. All these patients came daily to the hospital. Of course it is much better to keep the patient in a warm room.

Here are short notes of two cases, the first a very successful one.

J. H., aged seventy-two, has had a winter cough for the last three years. The cough comes on in fits, and is very bad at night. Fogs greatly aggravate it. She spits about a tea-cupful of thick yellow phlegm in the twenty-four hours. So bad is her breath that she cannot lie down at night, but is propped up with pillows, and is always wheezing. She is obliged to stay at home for weeks together. Her lungs are emphysematous, with only sonorous râles. After the first inhalation there was great improvement—freedom from cough all night, with much easier breathing. Further improvement took place after the next day's inhalation, and still more after the third, so that on the sixth day of treatment, and after three inhalations, she reported that her breathing "was not near so troublesome; thinks nothing of it now; does not spit up half as much," and the expectoration is white and frothy. This poor woman was loud in her praise of the treatment; said she "never expected it," and "when first she came to the hospital thought she should never get about again." J. H. is now sufficiently recovered to take charge of a shop, though before her attendance at hospital she had not been out of her room for four months. She was discharged, and called a month afterwards to say that there had been no relapse.

Now comes a less tractable instance, a fair specimen of one of the more obstinate cases.

M. A., aged thirty-two, came to the hospital January 29th with a winter-cough of many years' standing, and worse this winter than ever before. The cough is paroxysmal, the slightest exertion, even talking, bringing on an attack. The paroxysms vary much, but generally last ten minutes. In the twenty-four hours she spits about a teacupful of thick yellow phlegm. Extremely short-breathed, and she is quite unable to do her house work, and at night is unable to sleep unless propped up with three pillows and a bolster. The breathing always gets worse at night. Fogs increase all her troubles. Has been hoarse for weeks, and if she talks much she altogether loses her voice. Her chest is very sore with coughing. She is emphysematous, and her breath-sounds are obscured by cooing râles.

Feb. 3rd. The patient, who has had an inhalation on five successive days, now says she is in every way much better. The breathing is much easier: the cough is not nearly so violent; her chest is less sore; the expectoration is much less; and there is very little hoarseness.

6th. The inhalations have been continued daily. The patient says she is better than she has been all the winter. The improvement in her breathing is very great, and she can now do with only one pillow at night instead of three. She sleeps much better. The cough is greatly improved, and, instead of being "aggravated" towards night, is now better at that time. Expectoration has almost ceased.

10th. Has had only one inhalation since last date, and her breathing has been a little more distressed.

12th. Has had an inhalation daily, and the dyspnoea has again nearly disappeared.

17th. Has had but one inhalation since last date. The cough now has almost left her, and she often goes twelve hours without a fit. Her breathing is so much better that she now does her own house-work, and is not propped up at night.

Discharged after ten inhalations and nineteen days' treatment.

A month afterwards she came to the hospital to say that her breathing was all right, and that she had been perfectly well since her discharge, with the exception of a slight hacking cough.

All but one of the twenty-five patients were benefited. In one case the improvement was very gradual, but there was evident temporary improvement after each inhalation. In twenty-one cases the average number of inhalations required was 9.4, and the average number of days was twelve, before

the patients were discharged cured. The greatest number of inhalations in one case was eighteen, and the smallest three. The case longest under treatment required twenty-four days; the shortest, four.

In employing the ipecacuanha spray, in order to ensure as far as possible only its topical effects, we were careful to direct the patient to spit out and even to rinse out the mouth at each pause in the administration, for a much larger quantity of the wine collects in the mouth than passes into the lungs. If this precaution is not adopted, sometimes enough is swallowed to excite nausea and even vomiting, by which means the bronchial mucus is mechanically displaced, and of course in this way effects temporary improvement. Even when this precaution was observed, a protracted inhalation will excite nausea and sometimes vomiting by the absorption of the wine by the bronchial mucous membrane; though, strange to say, when thus induced, vomiting was long delayed, even for several hours—nay, sometimes not till the evening, though the inhalation was used in the morning. In the reported cases, however, improvement was not due to the nauseating effects of the spray, for we took care to avoid this contingency by administering a quantity inadequate to produce this result. The duration of each inhalation will depend on the amount of spray produced by each compression of the elastic ball, and on the susceptibility of the patient to the action of ipecacuanha. As a rule, the patient at first will bear from twenty squeezes of the spray without nausea, and will soon bear much more. After two or three squeezes, especially on the commencement of the treatment, we must pause a while. It is necessary to look at the patient's tongue and tell him to learn to depress it, for if the tongue is much arched it will hinder the passage of the spray to the lungs. It is a good plan to tell the patient to close his nose with his fingers and to breathe deeply. The inhalation should be used at first daily, and in bad cases twice or thrice in the day; afterwards every other day suffices, and the interval may be gradually extended. If the ipecacuanha wine is diluted, then the spray must be used a longer time. In cold weather the wine should be warmed.

We have tried the spray with very satisfactory results in a few cases of the following more severe though closely allied disease:—A patient for several years has suffered from severe winter cough, with much dyspnoea, cough, and expectoration; and on several occasions has spat up a considerable quantity of blood. The physical signs denote slight fibroid consolidation, with excavation of both apices, and much emphysema, perhaps atrophous in kind. There is little or no rhonchus, and no fever. The expectoration may be slight or very abundant,

muco-purulent or purulent. The dyspnœa is, perhaps, very severe; and is so paroxysmal as to justify calling the case bronchial asthma, with emphysema, and fibroid phthisis. In these cases the ipecacuanha spray is almost as beneficial as in the preceding. It soon controls the dyspnœa, thus enabling the patient to sleep, and greatly lessens expectoration and cough; and by these means really improves the general health. As in the previous cases, the first inhalation may considerably improve the breathing, though the effects are not so permanent, the dyspnœa returning in the evening; so that spraying is needed night and morning, and may be necessary for weeks or months, the ipecacuanha appearing rather to give relief than to permanently cure the dyspnœa.

We have used the spray in two cases of true and severe bronchial asthma, with very opposite results. In one severe case, accompanied by a great deal of bronchitis, it gave very great relief. The other patient, not so ill, has been all his life asthmatical; and on catching even a slight cold his breathing becomes greatly oppressed. In this instance each application of the spray considerably aggravated the dyspnœa, even when the wine was diluted with an equal quantity of water. Possibly a still weaker solution might have been borne; but we are inclined to think that in this case any quantity of ipecacuanha would have disagreed, as the tightness of breathing increased almost immediately the inhalation was begun.

The successful case was a very severe one. For years this woman had suffered from bronchitic asthma, and when she applied to the hospital was unable to lie down owing to shortness of breath. She suffered also from violent paroxysmal dyspnœa, the worst attack beginning about three a.m., compelling her to start out of bed and struggle for breath. She was very emphysematous; her voice was very hoarse. The first inhalation removed the hoarseness, and much improved her breathing, which continued freer till midnight, when the dyspnœa returned. The cough was eased, and she expectorated more freely. Each inhalation always gave her very great and marked relief. She walked to the hospital with great difficulty, and was constrained to stop frequently. On entering the room she could not speak, but laboured violently and with loud wheezing to get her breath. A few inhalations would gradually set the breathing free, so that the air entered more and more, and the wheezing gradually left, till, on the completion of the inhalation, she could breathe without difficulty. As the breathing improved she could feel the spray descending lower and lower in her chest. At first it would seem to reach only the back of the tongue, then the top of the

sternum, then descend to midsternum, and at last she felt as if it reached as low as the pit of the stomach. This improvement was maintained through the day, but at evening a relapse would occur, so that her nights, though at first bad, were still better than before the treatment. Soon, however, the effects became more lasting, and she slept well. On discontinuing the spray, however, her breathing again grew worse, and she was obliged to revert to the treatment; but unfortunately she so soon caught cold, and so bad was the weather, that she was obliged to stay away for days together. Whilst her breathing improved the cough and expectoration also improved, but these two symptoms continued rather troublesome. Probably in bad bronchitic asthma the spray must at first be used twice a day or oftener, and must be continued for some time to ward off the dyspnoea, for in these obstinate chronic cases the bronchitis may take a considerable time to cure. So marked was the improvement from the spray that the patient and her friends expressed their astonishment, especially at the prompt relief it gave.—*Lancet*, Sept. 5, 1874, p. 338.

DISEASES OF THE ORGANS OF DIGESTION.

33.—ON FOOD AND DIETETICS.

By Dr. F. W. PAVY, Physician to Guy's Hospital.

[The following is from a review of Dr. Pavy's recently published work "on Food and Dietetics."]

Of paramount importance and interest just now is the question—what is the true value of nitrogen-containing food supplies? That they must in some measure be required for tissue-building is, of course, obvious, more particularly in the young growing subject. But in what way are we to look at nitrogenised foods as contributing to force-production, now that we are aware that muscular force is not derived from the destruction of muscular tissues? Does urea, for example, represent the outcome of nitrogenised food which has been through a process which developed force applicable to vital purposes? Yes, says Dr. Pavy; but the urea itself is pure waste, and it includes nearly all the ingoing albumen of the food. His explanation is the following:—The nitrogen of albumen is about 15·5 per cent., and in passing out of the system carries with it certain proportions also of carbon, hydrogen, and nitrogen, which go to make up urea. There remains from the destruction of albumen about 66·80 per cent.

of its original weight, containing 46·86 carbon, 4·79 hydrogen, and 13·15 oxygen, besides sulphur and phosphorus for utilisation and exit in another way. Urea is the inutilisable portion of the albuminous principle, but "whether it is formed as a primary product of the splitting up of albumen—that is, whether the elements at once group themselves from the albuminous compound into the combination representing it—or whether it forms the final product of a series of changes, cannot be stated." In the next paragraph to this, Dr. Pavy puts forward what may be called an argument from final causes, when he suggests that urea, which is known to be so closely allied to carbonate of ammonia, is an alternative form of the latter (one of the normal last products of organic decomposition) *designed* for the safety of the organism. Urea can pass out of the body without doing harm. Carbonate of ammonia developed in large quantities would be so powerfully irritant as to seriously endanger life.

The final conclusion to which Pavy comes as to the position of albumen as a force-producer will interest our readers. He calculates that albumen has about half the value of fat, and a greater value than either starch or sugar, as a source of force. He puts aside as needlessly visionary Liebig's latest theory (developed under the pressure of circumstances which had compelled the latter to confess that urea is not found in the muscular tissues), that there is some hidden source of force stored up in nitrogenised compounds, just as alcohol gives out in combustion more heat than its corresponding amount of sugar, though some force has been already evolved in the process of fermentation.

A variety of more or less important consequences flow from the position which Dr. Pavy (in substantial agreement with the majority of recent physiologists) assigns to nitrogenised foods. Among the most striking of these (which has for some time past become manifest) is the exaggerated value which common opinion attaches to animal food as a main article of diet. Dr. Pavy, though no vegetarian, argues strongly on this point, and on the whole we agree with him. Animal flesh, so far as it has any special (physiological) claim to be admitted into our diet, has for a long time seemed to us to be chiefly recommendable on the score of its felicitous blending of a small amount of nitrogen with a great deal of fat. On all sides the oleaginous constituents of food are acquiring daily a higher recognised value; and a vegetarian who would take care to include a proper amount of these in his *menu* would have much to say for himself. But we would remark here, that Dr. Pavy, no more than any other physiologist whom we have studied, makes any clear or precise estimate of one important matter, viz., the

difference between the ages of *growth* and those of stationariness and decline, as regards the necessity for nitrogenous food-supplies. He does, however, make some forcible remarks on the necessity of frequent food-supplies for children.

In relation to the *rôle* of alcohol within the organism, we are glad to observe that Pavy admits, what we have so long contended for, that probabilities (according to the most recent discoveries in physiology) are on the whole in favour of the belief that alcohol is utilised as a force-producing food. Upon one point we are glad to have an opportunity of correcting an apparent misconception. He appears to reckon Anstie among those who believe that alcohol develops sensible heat and raises the temperature of the body. The experiments which he quotes (elevation of temperature in the ears of alcoholised animals) have long been recognised by Anstie as merely affording an instance of altered *distribution* of heat, owing to vasomotor paralysis.

On the whole, it would seem as if we were coming very near to the old Hebrew ideal of a plenteous and nourishing diet. "Corn," if we may include a few other vegetables with it, "wine, and oil" seem to make up a very natural and very efficient diet. Flesh-feeding is mainly an affair of economy of bulk, and by suitable devices, may be largely dispensed with.—*Practitioner*, July, 1874, p. 30.

34.—ON THE TREATMENT OF FUNCTIONAL DERANGEMENTS OF THE LIVER.

By Dr. C. MURCHISON, LL.D., F.R.S., Physician to St. Thomas's Hospital.

1. *Diet*.—[In the treatment of functional diseases of the liver the most important thing is the diet, by errors in which, in fact, the disease frequently first takes place. Habitual lithæmia ought to be corrected by avoidance of heavy meals and saccharine and oleaginous articles of diet. A minimum of albuminous food should be taken in order to produce the least uric acid; and a minimum of carbonaceous food in order to allow the uric acid to be oxydised as much as possible.]

Great caution is necessary in all forms of lithæmia, as to alcoholic stimulants. Malt liquors, port wine, champagne, and many other wines, ought to be strictly prohibited. Claret, or a small quantity of spirit largely diluted, as a rule, answers best; but even these should be taken sparingly, and many patients do best with no stimulants at all. This is not the occasion to discuss whether alcohol is necessary for the nutrition of the body in persons subjected to much mental or bodily toil, or

whether, taking the masses in all walks of life, the standard of health would be better maintained by teetotal habits or by a moderate use of alcohol. What I desire now to insist upon is, that alcoholic drinks in quantities usually regarded as compatible with, if not conducive to, health, and far short of what are necessary to affect the brain, in many persons undermine the foundations of health by deranging the liver; and that, to some individuals, even very small quantities are injurious. In persons who have been indulging largely, the risk of a sudden withdrawal of stimulants is less, I believe, than is commonly imagined. Unless there be evidence of a very weak heart, which itself may be the result of alcohol, the only unpleasant effects of sudden and complete abstinence, in my experience, have been sensations of sinking at the epigastrium and craving for alcohol, which a repetition of the stimulus has only temporarily relieved and has rendered more persistent.

2. *A Free Supply of Oxygen*.—Next to careful regulation of diet, this is the most important object to be aimed at in the treatment of functional derangement of the liver, and especially in that which induces imperfect disintegration of albumen. An excess of fresh air, indeed, will often counteract the bad effects of too large a quantity of food. Although recent observations, more especially those of Parkes, have shown that the common impression that muscular exercise materially increases the elimination of nitrogen from the body, is erroneous, there can be no doubt that exercise in the open air quickens the circulation of blood through the liver, in the manner already explained, and promotes oxydation, and that, by thus preventing the accumulation in the system of the imperfectly oxydised products of albumen, it operates beneficially in the treatment of functional derangement of the liver attended by lithæmia. The observations of Beneke and other authorities have shown that *sea-air* is an oxydising agent of great power, and that nitrogenous and sulphur-holding tissues more rapidly disintegrate under its influence. Accordingly, we find that many patients with hepatic derangement and lithæmia derive immense benefit from residence at the sea-side and sea-bathing, although unfortunately the good effects of sea-air are sometimes more than counterbalanced by unhealthy lodgings or improper and badly-cooked food.

3. *Aperients; Cholagogues*.—In a large number of cases of functional derangement of the liver, great advantage is derived from the frequent use of aperient medicines, whether there be a tendency to constipation or not. Aperients bring away not merely bile, but the products of disintegration contained in the fluid circulating between the liver and bowel prior to their further elaboration and elimination by the lungs and kidneys.

Saline aperients, from the promptness of their action and the large quantity of watery exhalation from the bowel which they bring away, are among the best for the purpose now mentioned. Recourse is usually had to the sulphate of magnesia (Epsom salt), the sulphate of soda (Glauber salt), or the tartrate of potash and soda (Rochelle salt), or to various combinations of these salts with chloride of sodium, carbonate of soda, and other alkaline salts, such as are found in the mineral waters of Carlsbad, Friederichshall, Püllna, Harrogate, or Cheltenham, or in the recently discovered Hungarian spring Hunjadi János. Daily experience shows the great benefit derived by patients with lithæmia from a course of one or other of these mineral springs, or from some artificial imitation of them, all of which are best taken with warm water, and in the morning fasting. All of these salts have little or no affinity for animal textures, so that they excite few changes in them; they cause very little irritation of the mucous membrane of the bowel, and do not excite peristalsis, so that they purge without producing griping or pain. They act apparently by preventing the reabsorption of the fluid which is constantly being exhaled from the blood-vessels in the bowel.

There are certain other aperients which have long enjoyed a great reputation for promoting the secretion and discharge of bile, and otherwise acting beneficially in derangements of the liver, and which have accordingly been designated *Cholagogues*. Among these remedies, *mercury* and its preparations hold a pre-eminent place. At the present day, mercury has lost much of its former reputation as a cholagogue and alterative, and there is much difference of opinion as to its power over the liver. The practical physician gives a dose of calomel, finds the quantity of bile in the motions greatly increased, and his patient's state much improved; and he argues that the liver has been stimulated by the mercury to an increased secretion of bile, and that to this cause his patient's improvement must be ascribed. The physiologist, on the other hand, ties the common bile-duct in one of the lower animals, produces a fistulous opening into the gall-bladder, and then finds that calomel has no effect on, if it do not diminish, the amount of bile that drains away through the fistula. It may interest some who are present, if I refer briefly to the principal of these experiments.

Köliker and Müller, in 1855, tried the effects of calomel upon the secretion of bile in a dog with a biliary fistula. The results were somewhat contradictory. Once the bile seemed to be increased, and twice it seemed to be diminished, by the administration of calomel.

Of four experiments made in 1858 on a dog with a biliary fistula, Dr. George Scott found that in all the administration

of large doses of calomel was followed by a diminution of fluid bile and of bile solids.

In the same year (1858), Dr. Mosler made similar experiments upon two dogs with biliary fistulæ. The administration of calomel was not followed by any increase of bile, nor could mercury be detected in the biliary secretion.

Ten years later (1868), a committee of the British Medical Association, with Professor Hughes Bennett of Edinburgh as chairman, made a number of similar experiments on dogs, and came to the conclusion that "mercury did not increase the flow of bile, but rather diminished it."

The most recent experiments are those of Dr. Röhrig of Kreuznach, made in the Pathological Institute of Vienna. He found that, although large doses of calomel did seem to increase somewhat the secretion of bile, its power to do so was inferior to that of croton oil, colocynth, jalap, aloes, rhubarb, senna, and sulphate of magnesia, the cholagogue power of these drugs diminishing very much in the order in which they have now been enumerated, and calomel standing at the bottom of the scale.

These results of experiments upon the lower animals have added greatly to the discredit previously thrown upon mercury by its failure, when brought to the test of accurate clinical observation, to absorb plastic lymph in most forms of inflammation; and some eminent physicians are even of opinion that mercury and its preparations ought to be erased from our Pharmacopœia. On the other hand, it has been fairly objected that the results of experiments with mercury upon dogs do not warrant conclusions as to its effects upon man; and, even granting that in man mercury does not increase the quantity of bile secreted by the liver in health, it does not follow that in disease there may not be some condition adverse to the formation of bile, which mercury may have the power of removing. Much, however, of the difference of opinion between the physiologist and the practical physician may be reconciled by keeping in mind the osmotic circulation, as constantly going on between the intestinal contents and the blood. A large part of the bile secreted by the liver and thrown into the bowel is constantly being reabsorbed, to reach the liver again; and accordingly when the common bile-duct is tied and a fistulous opening into the gall-bladder established, the quantity of bile which escapes from the fistulous opening immediately after the operation is much greater than at any time subsequently (Schiff). Mercury and allied purgatives produce bilious stools, by irritating the upper part of the bowel and sweeping on the bile before there is time for its reabsorption. The fact of mercury standing at the bottom of the scale of cholagogues in Röhrig's

experiments is accounted for by it surpassing other cholagogues in this property; for, of course, the larger the quantity of bile that is swept down the bowel, the less is reabsorbed and the less escapes from a biliary fistula. That mercury does act especially upon the duodenum, is proved not merely by the large flow of bile which follows its action, but by the fact discovered by Radziejewski, that leucin and tyrosin, which are products of pancreatic digestion, under ordinary circumstances decomposed in the bowel, appear in the fæces after the administration of mercurials. It would appear, then, that mercury, by increasing the elimination of bile, and lessening the amount of bile and of other products of disintegrated albumen circulating with it in the portal blood, is after all a true cholagogue, relieving a loaded liver far more effectually than if acted merely by stimulating the liver to increased secretion, as was formerly believed, and as some authorities still maintain; for in this case it might be expected to increase, instead of diminish, hepatic congestion. It is not impossible, also, that the irritation of the duodenum by calomel and other purgatives may be reflected to the gall-bladder, and cause it to contract and discharge its contents, and thus account in part for the increased quantity of bile in the stools.

There are also, I think, grounds for believing that, apart from its increasing the discharge of bile from the bowel, mercury exerts a beneficial action in many functional derangements of the liver, in whatever way this is to be explained. Patients of the greatest intelligence suffering from hepatic disorders constantly declare that they derive benefit from occasional or repeated doses of mercurials, which no other medicine or treatment of any sort confers; and the scepticism of the most doubting physician would, I believe, be removed, should he unfortunately find it necessary to test the truth of their statements in his own person. It is not impossible that the good effects of mercury on the liver, and in some forms of inflammation, may be due to its property of promoting disintegration. Mercury appears to have the power of rendering effused fibrin less cohesive, and so more easily removed by absorption, than it otherwise would be. Modern physicians of high standing, and little likely to be accused of credulity as to the beneficial action of drugs, have thought that mercury is useful in croup, by causing a degradation and disintegration of the plastic membrane. If this be so, it seems not improbable that mercury, which from experiments we know to reach the liver, may under certain circumstances act beneficially by promoting, or in some way influencing, the disintegration of albumen. The remarkable effect of mercury on constitutional syphilis probably admits of a similar explanation. But in whatever way it is to

be explained, the clinical proofs of the efficacy of mercury in certain derangements of the liver are to my mind overwhelming. I say so the more advisedly, because I was taught to regard mercury as a remedy worse than useless, not only in hepatic diseases, but in syphilis; it cannot, therefore, be said that the convictions forced upon me by experience are the result of preconceived opinions.

Podophyllin is a remedy which seems to act in a very similar manner to mercury. Dr. Anstie's experiments with it on dogs and cats show that it has a special affinity for the small intestines, and especially for the duodenum. So far as my experience goes, it is less certain in its action, and even in moderate doses more likely to cause griping and mucous stools, than the preparations of mercury. It is a good substitute, however, for mercury, when from any cause this is contraindicated.

Colocynth, *Aloes*, *Rhubarb*, *Senna*, and *Jalap*, are also useful aperients in functional derangements of the liver resulting in lithæmia, constipation, or deficient excretion of bile. The experiments of Röhrig seem to show that they actually increase the amount of bile secreted by the liver. *Colchicum* also is an useful adjunct to other aperients in cases of liver derangement with lithæmia. According to Dr. Garrod, it "may often be given with advantage to gouty subjects as a cholagogue in lieu of the preparations of mercury," which, I may add, are often contraindicated in chronic gout with renal disease.

With these remedies we may include *Taraxacum*, which has long been thought to exercise a specific action upon the liver, but which probably acts mainly as a mild aperient. When there is a tendency to constipation, it may be advantageously combined with either alkalies or mineral acids.

4. *Alkalies*.—Next to aperients, alkalies are the most useful drugs in the treatment of functional derangements of the liver. In lithæmia and in many of the symptoms which spring from this morbid state, the greatest benefit is often derived from a course of alkalies—such as the alkaline salts of potash, soda, or lithia, or some of the alkaline mineral waters, such as those of Vals, Vichy, or Ems. The comparative worth of the different alkalies for neutralising acids varies considerably. One grain of carbonate of lithia or of carbonate of ammonia is nearly equal to a grain and a half of carbonate of soda or two grains of carbonate of potash. The beneficial effects of alkalies in derangements of the liver are not due to their neutralising acidity, or to any direct action upon lithic acid. It is, in fact, in the form of lithate of soda that lithic acid is met with in gouty persons. Alkalies seem to do good by combating the pathological state, on which the formation of lithic

acid depends. They are believed to promote oxydation, and thus increase the disintegration of albumen. Dr. Bence Jones tells us that in the body as well as out of it alkalies furnish the most marked evidence of assisting in oxydising actions. The experiments of Parkes with liquor potassæ seemed to show that it had the power of increasing the disintegration of the sulphur-holding materials of the body. The effect of its administration was to increase the amount of sulphuric acid and also of urea in the urine; although, with characteristic caution, Parkes adds that the increase of urea as the result of the potash was rendered probable, rather than proved, by his experiments. From experiments on dogs with biliary fistulæ, Nasse was led to the conclusion that carbonate of soda taken with the food diminished greatly the secretion of bile, and a similar result has been observed by Röhrig to follow the introduction of the same salt into the intestine or the veins. The diminution affected the solids as well as the water of the bile, and especially the biliary salts. Nasse also found that after taking two drachms of carbonate of soda the urine (human) was very rich in hippuric acid. The only inference at present to be drawn from these experiments is, that alkalies exert a powerful influence over the chemical changes going on in the liver. When alkalies are employed in lithæmia, it is well to suspend their use occasionally, as they are apt, when long continued, to derange the gastric digestion; but in cases where they are strongly indicated they are better tolerated than is usually thought. In the fifth volume of the Medico-Chirurgical Transactions, Dr. Bostock has recorded the case of a young lady who for months took carbonate of soda to the amount of two ounces and a half daily. The appetite and strength were much improved; and her blood, instead of being thin, coagulated firmly, the coagulum being strongly buffed and cupped.

5. *Chlorine, Iodine, and Bromine* are closely related in their chemical properties, and are believed to promote oxydation in the body by taking hydrogen from water and liberating oxygen. An aqueous solution of chlorine is of service in certain cases of lithæmia associated with general debility; and we know that the various salts of chlorine enter largely into the composition of the mineral waters which are most useful in hepatic derangements. Bromide of potassium will reduce certain enlargements of the liver and spleen, and may be given with advantage in cases of lithæmia associated with congestion of the liver and want of sleep. But among the remedies of this class the chloride of ammonium holds a pre-eminent place. It has obtained a great and well deserved reputation in India and other tropical countries for the treatment of hepatic congestion; and I have found it of great service in the functional derange-

ment of the liver attended by lithæmia. Given in scruple doses three times a day, it acts as a diaphoretic and diuretic, and exercises a powerful influence in relieving the portal circulation. It is not oxydised, but passes out of the system unchanged in the urine. According to Böcker's experiments, it increases the nitrogenous solids of the urine; the mean daily increase of urea under its use he found to be not less than seventy-four grains—a quantity indicating a vast augmentation either of metamorphosis or of elimination, but from its beneficial effect on the liver, most probably of the former. Chloride of ammonium has this advantage, that it may be combined with either alkalies or mineral acids.

6. *Mineral Acids* are employed by many physicians in the treatment of functional derangements of the liver. Nitric acid especially has long been thought to have the power of augmenting the flow of bile; but there is no good evidence of this, either clinical or experimental. According to my experience, the action of mineral acids upon the liver is much less direct than that of alkalies. In all morbid states of the liver attended by congestion, and in most cases of lithæmia, I have found that they either did no good or they aggravated the symptoms. They may, however, be of service when there is debility and want of tone; but the chief good which they effect is probably that of improving the gastric digestion. In some cases, both acids and alkalies may be given advantageously—the alkalies before, and the acids after, a meal.

7. *Tonics*.—Clinical experience shows that, notwithstanding the existence of debility and anæmia, tonic remedies are apt to disagree in many cases of functional derangement of the liver. This remark applies especially to the functional derangement resulting in lithæmia. I have repeatedly known patients in this state improve at once on substituting for quinine, iron, the mineral acids, and stimulants, abstinence from alcohol, with aperients, blue pill, and alkalies, and careful regulation of diet; the strength, flesh, and colour returning under what at first sight might have appeared a lowering treatment. Different opinions have been expressed with regard to the utility of iron in chronic gout. According to Dr. Bence Jones, iron is one of the two potent remedies we possess for indirectly promoting oxydation in gout as well as in other maladies; whereas, in Dr. Garrod's opinion, the preparations of iron are very likely, when indiscriminately given to gouty subjects, to excite a paroxysm of the disease, and for the most part are contraindicated. My experience coincides with that of Dr. Garrod; and in simple lithæmia I have constantly known iron to increase the tendency to deposits of lithates in the urine, constipate the bowels, and aggravate any symptoms from which the patient may have

previously suffered. As a rule, also, I have found that patients with chronic gout or with lithæmia do not tolerate even small doses of quinine. From some careful experiments made by Dr. Ranke of Munich, quinine appears to have the power of diminishing the amount of lithic acid in the urine. The experiments were made on three persons, and the results were uniform. The effect continued for about two days after a single dose of twenty grains; and there was no evidence of any increased excretion after the effect of the quinine had passed off, so that probably the quinine acted by lessening the formation of lithic acid in the liver, or by substituting some other substance for it. From these experiments it might be inferred that quinine and bark should be of essential service in chronic gout and in lithæmia, but this inference is opposed to clinical experience.

When tonics are given in lithæmia, gentian, chiretta, cascarrilla, and serpentaria are preferable to quinine and other preparations of bark. The best preparations of iron are the reduced iron, the citrate of iron, or the tartrate of iron and potash; these preparations are sometimes advantageously combined with alkalies and saline aperients. In cases of lithæmia attended by great nervous prostration, I have seen advantage follow the use of phosphorus in doses of one-thirtieth of a grain three times a day. The lithates have disappeared from the urine, and all the symptoms have improved. In some of these cases the circumstances seemed to leave no doubt that the improvement was due to the phosphorus.

8. *Opium* and its preparations are contraindicated in most functional derangements of the liver, and particularly when there is evidence of lithæmia. Opium impedes elimination both by the bowels and kidneys, and also appears to check the disintegrative processes which go on in the liver. It is generally believed to diminish the amount of bile secreted by the liver; and this view is favoured by the light-coloured stools which often follow its use. The result, however, of Röhrig's experiments on animals with biliary fistulæ was to show that opium increased the secretion of bile instead of diminishing it, so that probably the discharge of bile from the liver is only temporarily suspended by a similar influence on the coats of the bile-ducts to that which opium exerts on the coats of the bowel. Be this as it may, there can be no doubt that opium constipates the bowels, favours portal congestion, and checks the elimination, not only of bile, but of the products of disintegration which go on in the liver. The experiments of Böcker and clinical experience alike show that opium impedes the elimination of the nitrogenous solids of the urine, and that dangerous and even fatal consequences ensue from its employment in

structural diseases of the kidneys. These considerations explain why, in hepatic derangement attended by lithæmia, opium is contraindicated for the relief of pain, sleeplessness, or other symptoms for which it is commonly prescribed.

On the other hand, in the functional derangement of the liver which exists in diabetes opium is tolerated in large doses, and is often of signal service in checking the formation of sugar. Its good effect is probably in great measure due to some influence on the vaso-motor nerves of the hepatic vessels, a reflex paralysis of which we have found to be one of the causes of diabetes.—*British Med. Journal*, May 2, 1874, p. 568.

35.—SOME POINTS IN THE DIETETIC TREATMENT OF DISEASE.

By Dr. E. A. PARKES, F.R.S.

While making some experiments on soldiers at Netley with different kinds of food I have come across some facts which may be of interest in a practical point of view, or will at any rate elicit the views of members of the Society on the subject of diet in disease.

1. The first point is *the production of dyspepsia and its relief by diet*.

I was at one time experimenting on two men with a diet of dried meat and bread, no vegetables and no fats being given. The object was to see if soldiers could be kept in health on such a diet for two or three weeks, as in that case a man would be able to carry almost enough food to last him, it may be, all the time of a rapid expedition, such as the march to Coomassie; at any rate, the amount of waggon transport required would be greatly lessened. The meat was good and was merely dried, so that it contained only 10 or 12 per cent. of water instead of 70; the bread was good and of the ordinary kind. In both men this diet produced in two or three days very great indigestion and depression of spirits, and in both an eruption of little pimples, like acne, came out on the face and shoulders. In both cases the dyspeptic symptoms and the little eruption were at once removed, not by omitting the dried meat, but by adding starches and butter to the same diet. The illness produced by the one kind of food, and the immediate cure which followed the addition of alimentary principles previously deficient, seemed to me very important in connexion with dieting in disease, and especially in fevers, where often such quantities of animal broths are given, and, with the exception perhaps of a little arrowroot, no starches and no fats are ordered.

The relief to the dyspepsia of these two men by adding starches and fats was shown in another case.

An inventor sent to the War Office a dried food composed of meat, flour, potatoes, some other vegetables, and salt. It was supposed to contain all the essentials of a good diet, and it was thought that twelve or thirteen ounces would be a sufficient ration for a day, and that a soldier could carry enough for twelve days. Many foods of this kind have been proposed, and they have almost all failed on trial, owing, I believe chiefly to their being too highly dried. In the experiment I now refer to this food produced in two or three days the most violent dyspepsia, and made the man feel so uncomfortable and depressed that he declared he would rather starve than eat it. I therefore left it off altogether, and put him on a diet composed of arrowroot and butter deprived of all casein, and gave him no salt. The effect of this was perfectly marvellous: all symptoms of dyspepsia disappeared in twenty-four hours; the man at once recovered his spirits, and it was quite clear that the irritative condition produced by the concentrated food had been at once removed.

The treatment of "irritable gastric dyspepsia" by mild farinaceous and vegetable food is well known, but the rapidity of cure of these cases seems to me to show that the best way would be not to give vegetables at all, or any nitrogenous food, but for two or three days to let only starches and fats in some digestible form, and without salt, enter the stomach. I found these men could take from two to three ounces of butter in the shape of arrowroot biscuits, and also spread upon these biscuits, without producing any of the dreaded "bilious" effects usually attributed to fatty foods.

In reference to the little papular eruption produced in two persons by a diet too largely composed of meat, I think some interest attaches to a fact of an opposite kind. In 1867, Dr. Passavant, of Frankfort, published an interesting paper (*Archiv der Heilkunde*, 1867, p. 251) on the treatment of non-syphilitic psoriasis inveterata by means of a meat diet. He had himself suffered for twenty-five years from psoriasis, and had vainly tried every possible remedy. He was cured at last and completely by a meat diet; that is, by Bantingism, but without the alcohol allowed in that system. A case of eczema squamosum was also cured by this diet.

Just after I read this I was asked to see a case of most inveterate psoriasis, in consultation. I found every remedy I suggested had been tried. At last, in despair, I mentioned Dr. Passavant's observation, and it was agreed to try the meat diet without any medicine. The patient rapidly recovered. I have had no other case since in which it could be tried, and I men-

tion the fact chiefly to know if the plan has been used by others, partly because in my two cases of papular eruption the meat diet also appeared to have a special influence on the skin, though it was in the contrary way—viz., it produced instead of cured a disease.

2. The next point I wish to bring before the Society are some other effects of a non-nitrogenous diet.

I have noticed two conditions to be produced by a purely non-nitrogenous diet, which may be useful in the treatment of disease.

Such a diet lessens in a very great degree the acidity of the urine. We know that the free urinary acidity is usually attempted to be lessened in two ways: first, by giving alkaline medicines or drugs which become alkaline in the body, as the acetates or citrates; and, secondly, by giving fresh fruits and vegetables, which contain similar salts. Now, in both these plans, it has appeared to me that the acid is merely neutralised, and that its formation is not prevented. On the contrary, I have formerly given some evidence to show that the taking of alkalis may really increase the acid-building processes in the body. What these two plans do, I take it, is to neutralise a urine unduly acid, and thereby render it less irritating to the urinary surfaces; and this is a great service, only that it is not all that is sometimes required.

A non-nitrogenous and non-saline diet lessens the formation of acid, as the following facts will show:—

The normal amount of free acidity in the urine is usually expressed, as proposed by Julius Vogel, as equal to so much crystallised oxalic acid; the normal daily average amount of free acidity in the urine of healthy men is equal to the acidity of from two to four grammes of crystallised oxalic acid.

I kept a healthy man on ordinary meat diet for fourteen days. The mean free acidity was equal to 2·749 grammes of oxalic acid in twenty-four hours; the highest amount on any one day was 3·369 and the lowest 2·122 grammes. I then put him on a diet entirely non-nitrogenous, and very nearly non-saline. On the first day the free acidity of the twenty-four hours' urine sank to 1·847 grammes, and on the second day to 1·255 grammes, or less than one-half the average.

I kept another man on ordinary diet for twelve days; the mean daily acidity was equal to 3·406 grammes of crystallised oxalic acid. He was then put on a non-nitrogenous diet for four days; the mean daily acidity of these four days was only equal to ·873 gramme of oxalic acid, or one-fourth the normal amount; on the third day it was only ·618 gramme, or one-sixth the normal amount. He then returned to meat diet, and subsequently was placed again for five days on non-nitroge-

nous, and non-saline food. On the first three days the mean acidity was only .509 gramme in each twenty-four hours; on the fourth day the amount was only .0368 gramme, and on part of the day the urine was alkaline; on the fifth day the acidity was only .444 gramme, or one-eighth the normal amount.

I notice also, in Dr. Pavy's excellent book on Food, that Mr. Mahomed in some experiments on a non-nitrogenous diet found his urine became alkaline.

Now to ensure this diminution of acidity there must be more than a mere farinaceous and milky diet; at least I found a man who took only oatmeal and milk had a free acidity equal to 3.022 grammes of oxalic acid on an average of many days. To produce the effect the diet must be non-nitrogenous, perhaps non-saline. This diet also increases the flow of urine, so that I am sure it must be found useful in gravel and uric-acid deposits generally.

I was curious to know whether when a man was on this non-nitrogenous diet he would pass any uric acid; the quantity of urea daily excreted was of course greatly lessened, and so was the uric acid, but even after three or four days of non-nitrogenous diet I could still always find a little uric acid in the urine. This seemed to me an argument against the origin of urea from uric acid; for why, when all nitrogen was cut off, and consequently the oxygen must have been in relative excess, should not all the uric acid have been converted into urea if that is its usual origin in the body? It was not so converted, however, but passed out *pari passu* with the urea as if furnished by special cells. At any rate we may be sure we cannot entirely exclude uric acid by a non-nitrogenous diet, though we can greatly lessen its amount.

The second effect produced by non-nitrogenous diet is a great lessening of the power of the heart, which is perceptible twenty-four hours after the commencement of the diet. The heart beats more feebly, the pulse is smaller and softer, and if the sphygmograph is used, the height of the up-stroke is only half of what it is in the normal state of the man. This action on the heart is so marked that it should make us watch the effect of a non-nitrogenous diet. It may be useful, I think, in two cases—first, when we wish to reduce the action of a powerfully acting and excited heart, and, secondly, in the treatment of aneurisms. The treatment of Valsalva by rest, bloodletting, and low diet, has been revived in a better form by Tufnell, who has met with great success. Perhaps the use from time to time of a perfectly non-nitrogenous diet, in addition to rest, might aid in lessening the force of the heart, and therefore increasing the tendency to coagulation in the sac.

The mode in which I gave the non-nitrogenous diet was as follows:—Butter was melted in a jug placed in a water-bath, and the liquid oil was poured off. Arrowroot cakes were made with a portion of this butter, and a little sugar was added. A good cook can thus make very agreeable cakes, which should not be in the least tough. These arrowroot cakes were eaten by my patients with great relish when buttered; they also took arrowroot jelly sweetened.

The healthy men on whom I experimented ate as much of this food as they liked; they took on an average ten ounces of arrowroot, six ounces of sugar, and two ounces and one-third of butter, in each twenty-four hours. This quite satisfied hunger, and maintained weight; and all four men on whom I made these trials liked this diet, and felt perfectly well on it. The longest time I kept a man on it was five days, and he did a hard day's work on the fifth day.

If it is wished to add a little nitrogen to suit a diet, but not to give meat or bread, it is best done by substituting ground rice for some of the arrowroot and sugar; one ounce of rice contains three grains and a half of nitrogen. If more nitrogen is desired, then it may be given in the form of eggs (yolk and white); an egg of two ounces in weight contains nine grains and one-third of nitrogen.

The influence of this diet of arrowroot and butter oil on the free acidity of the urine is very interesting. It shows us that something still remains to be solved, even after the important observations of Bence Jones, William Roberts, and J. Vogel. It is clear that the free acidity of the urine is unconnected with the changes in the body of the starchy and oleaginous aliments. The lactic acid formed by the one does not, we know, appear in the urine, and the fatty acids coming from the other appear also to have no effect.

Some part in the diminution of the urinary acidity must be attributed to the absence of the phosphates which enter so largely into the meat, milk, and cereal diets, and which, by their peculiar properties, play so large a part in giving acidity to the urine. Could the want of choride of sodium have had any effect in this way? Of this I am not clear, but as the hydrochloric acid of the gastric juice is no doubt derived from common salt, and as in this diet there must be a great lessening in the amount of hydrochloric acid poured out in the stomach, I am disposed to think the rapid disappearance of irritable gastric dyspepsia may be in part connected with the want of salt in the diet.

The absence of nitrogenous aliment would necessarily check the formation of uric and hippuric acids, and thus also lessen the acidity of the urine. Whether the absence of the phos-

phates (which, passing in neutral, pass out acid) and the non-formation of uric and hippuric acids will explain all the diminution in the free acidity of the urine, or whether other acids are ordinarily formed in the metaphorphoses of the nitrogenous tissues, which are not formed on a non-nitrogenous diet, is a point on which it is at present hazardous to express an opinion.

What is the influence of alcohol on appetite and digestion?

Few more important questions could be asked by men concerned in the treatment of disease, but so difficult is the answer that no writer I know of treats it fully.

It is, I believe, generally admitted that in large doses alcohol lessens, and at length destroys, appetite. It is very desirable to know if this is the case, and, if so, what is the quantity which will thus act in healthy men. I have made some observations on this point, which I will now give.

A healthy man took, on successive days, one, two, and four fluid ounces of absolute alcohol in divided doses with water and food. The first two quantities did not lessen appetite, but, according to the man's statement, increased it. Four fluid ounces, however, greatly lessened appetite; and on increasing the quantity to six and eight fluid ounces appetite was entirely destroyed, and the man could eat nothing. These experiments showed that in this healthy man something between two and four fluid ounces of alcohol in twenty-four hours lessened appetite.

Another man took, on successive days, in one dose and before food, one, two, four, six, and then again six fluid ounces of undiluted brandy, containing about 50 per cent. of absolute alcohol. The one and two fluid ounces of brandy (equal to half an ounce and one ounce of absolute alcohol) had no perceptible effect on appetite one way or other; four ounces of brandy (equal to two fluid ounces of absolute alcohol) greatly lessened it; and six ounces of brandy (equal to three ounces of alcohol) destroyed it, so that the man could not force himself to eat all his food, though strongly urged and indeed anxious to do so. In this case the brandy was given three hours after breakfast and three before dinner.

To another healthy man six ounces of undiluted brandy were given on three days, three hours before food; and on three alternate days he took no brandy. The brandy contained 36 per cent. of alcohol by volume, and therefore the six fluid ounces contained a little over two fluid ounces of absolute alcohol. In this experiment the man was kept without food every day from 6 p.m. until 1 p.m. the next day, or for nineteen hours; he took the brandy therefore after sixteen hours' fasting. On the days when he took no brandy his appetite was excellent. On the brandy days, although he felt exces-

sively hungry before he took the brandy, he lost all sensation of hunger soon after it: in spite of urging he could not take all his food even after this long fast.

It seems, then, that alcoholic beverages containing about two fluid ounces of alcohol produced marked lessening of appetite in both those men who took the brandy on an empty stomach; and in the first man, who took the alcohol with food, something more than two ounces was required to produce this effect.

In a fourth healthy man twelve ounces of brandy taken in divided doses during the day, and chiefly with food, did not affect appetite, but the man informed me that he was himself well aware that a large quantity would do so, as he had often made the experiment on himself.

These observations may be considered in the light of fairly exact experiments on men whose digestion was perfect and who were quite healthy. In no case were the men prompted, nor did they know whether I expected that the appetite would be increased or lessened. Three of them thought the appetite would be increased; the fourth knew, from previous experience, that spirit-drinking largely destroyed appetite. I believe it may be safely concluded that anything over two fluid ounces of alcohol may lessen appetite if the stomach be empty, and therefore necessarily injures digestion.

The observations I have myself made on this point on cases of disease are, I am sorry to say, fragmentary. I have seen some patients who were having large quantities of alcohol and who complained that they could eat nothing, and on lessening or taking away alcohol the appetite has returned. In such cases I cannot but conclude that alcohol was acting hurtfully; for surely, in most cases of disease, one great object is to get nourishment of the ordinary kind into the system. But there are other cases in which men have taken considerable quantities of spirits and yet could eat well. I think the time when the spirit is taken may make the difference. In my healthy men the greatest injurious effect on appetite was produced when the alcohol was taken on an empty stomach, and the least when it was taken with food.

The injurious effects of such doses of alcohol will, I believe, be admitted; but now let me turn to the other side.

If alcohol in large quantities lessens, and eventually destroys, appetite, do small quantities increase it? Let me test this first by healthy men.

The man whose appetite was nearly destroyed by four fluid ounces of alcohol thought that one and two ounces increased his appetite. The man whose appetite was destroyed by four fluid ounces of brandy did not think that one and two ounces

had any effect either way. He was, indeed, disappointed with the effect of one ounce of brandy, as, after abstinence for many days, he had been looking anxiously for it, and thought he should enjoy his dinner more. In this case, then, half an ounce of pure alcohol had no effect; and, subsequently, one ounce of pure alcohol was not more potent in increasing appetite in this man. Would smaller quantities have been more useful? To this I can give no answer from experiment. In the case of the man whose appetite was not lessened by twelve ounces of brandy given in divided doses, there was no increase. In the fourth man the brandy was given in too large doses to test this point.

The evidence from these three healthy men as to any increase of appetite and more vigorous digestion from small doses of alcohol is therefore uncertain, and it is more negative than positive.

Turning, however, from these experiments to the general opinion derived from talking with patients and friends, my own experience is this:—Both healthy men and patients have told me that appetite has been really increased by taking spirits, wine, or beer, in small quantities with food.

I have two observations on this point which seem to be really exact. A relative of my own, and a man of singularly good judgment and honest mind, was a total abstainer for many years. When he was between fifty and sixty years of age he got out of health; his digestion became feeble; he ate less, and began to get thinner. He was advised to take one-half to one pint of beer with dinner. He did so unwillingly, but found to his surprise that he ate more, digested better, and soon recovered all his lost ground. He has continued taking beer once a day and occasionally one or two glasses of wine ever since. He is now seventy years of age, very healthy, and has no doubt that he has been greatly benefited by this alteration in diet.

The other case is even stronger, and will be interesting to all, because it occurred to my friend Dr. Carpenter, who has done more than any man living perhaps to advocate temperance. In his writings on this subject, Dr. Carpenter has always maintained that the action of alcohol on the human body when under par is different from its action on an individual in full health, and is beneficial in many cases with depressed digestive power. He has been so kind as to give me an account of his own case, and to allow me to quote it. He writes to me thus: “After having been a water drinker during all the earlier part of my life, and enjoying a fair measure of health and vigour, I broke down about ten years ago under the pressure of excessive work, and, besides a local disorder, I then suffered

from a total loss of appetite and enfeeblement of the digestive power, so that my whole system was undergoing a rapid lowering. My medical friends recommended me powerful tonics, combined with three glasses of sherry daily, and on this regimen I improved even more rapidly than they expected, and was able in a month's time to enjoy a tolerable dinner, gradually reducing the quantity of wine I took with it. They had at first expected that I should be obliged to winter in the south of Europe; but I rallied so fast that this idea was soon abandoned, and I was able to return to my work after a three months' absence. Ever since that time I have taken a couple of glasses of light claret with my dinner, and this fluid suits me very well. I often reach home very tired, and feeling as if I could eat nothing, and I am certain that without this little 'fillip' I should eat nothing. The question lies, therefore, in such cases between the use of the slight alcoholic stimulus and the inadequate nutrition of the body, and I cannot myself doubt which is the *least* of what I am willing to admit to be *two evils*."

Coming from such a man, this evidence seems to me indisputable, and, coupled with that derived from watching patients with weak digestions, I think may be called conclusive.

Then come the questions, What amount of alcohol increases appetite, when should it be given, and in what form?

With regard to the *amount*, I should say that, if I may judge from my own experience on healthy men and on patients, the quantity which should be given for this particular purpose of increasing appetite should not exceed one fluid ounce of absolute alcohol in a day, and I think half an ounce is often enough. If this does not improve appetite, a larger quantity will not do so. I am quite sure we cannot force appetite by increasing the amount of alcohol, but, on the contrary, shall run the risk of lessening it. Alcohol has sometimes to be given in large quantities to urge on a failing heart or to blunt sensation; but this is another thing. In such cases a paramount necessity compels us to disregard the question of appetite. But when, as in most cases, our object is to get more nourishment into the body, then we should keep, I believe, to small doses, and if the medicine fails, try something else. I must add to this, however, by affirming what I am quite sure of by observation, that there are persons whose appetites and digestions are injured by even small quantities of alcohol, and who digest much more and eat much more when water only is taken. The healthy digestion of children is, I think, always injured.

As to the *time* when it should be given, I have no doubt it must be with food. The reason of this is tolerably clear. The alcohol is mixed with the contents of the stomach, acts, there-

fore, in a very diluted form upon the mucous membrane, and is absorbed slowly. We may, therefore, safely infer that the excessive redness and dilatation of vessels, which a tolerably strong alcoholic solution causes in living tissues, is avoided; there is probably a moderate paralysis of the vaso-motor nerves and a more rapid circulation and pouring out of gastric juice. But give it without food or in such large quantities even with food as to impart to the fluids bathing the delicate cells and tubes of the stomach a more concentrated alcoholic composition, and we can believe that the albuminous tissues are damaged and the nerves and vessels are probably paralysed to an extent which interferes with the formation and pouring out of the gastric juice. If this be so—and I believe the facts indicate it—what madness it is for men to be drinking at all times of the day, especially spirits and strong wines, which must inevitably ruin in the long run the surfaces they bathe and the tissues they pass through in a comparatively concentrated state.

With regard to the *form* in which alcohol is given to promote appetite, I think if a fattening effect is also desired, and no gouty tendencies exist, beer containing a good deal of malt extract is the most suitable; if salines and acids seem to be desirable for the kidneys, then the light French wines seem a good form, though many persons of weak digestion are unable to tolerate the quantity of acid and acid salts most of these wines contain. In certain cases the stronger astringent wines may evidently be best, and in all cases, no doubt, a natural wine, as little doctored as possible, and free from the chance of other alcohols or of fusil oils, should be ensured. But I confess I am quite unable to know on what grounds such minute distinctions are so often made in recommending wines—why one should be proscribed, and the other prescribed, although practically the same. Purity, alcoholic strength, and the amount of free acidity, salts, and extract, are surely the most important points to guide us in prescriptions, for the ethers are in too small a quantity to have much effect on digestion, though the aroma and bouquet they aid in giving may be pleasant.

For many patients, when the effect of alcohol on digestion only is sought, I think by far the best plan is to follow the plan recommended by Wilks, and to give rectified spirits, properly disguised, as a medicine; we shall then be certain of purity, that proper quantities are given, and at the time we desire; that no discretion is left to nurses or friends, and that the chances of a bad habit being formed are less.

I have confined myself in these remarks to one of the actions of alcohol, the importance of a correct knowledge of which

can scarcely be overrated. I have not desired to enter into the question of the action of alcohol on the heart, nervous system, or other parts, but simply to commence a discussion on this one theme—viz., the influence of alcohol on appetite and digestion, and the consequent nourishment of the body.—*Lancet*, May 23 and 30, 1874, pp. 722, 758.

36.—THE TREATMENT OF ROUND WORM.

By Dr. THOMAS E. CLARK, Clifton.

A peculiar case came under my care as an out-patient at the Bristol General Hospital in February last. The patient, a child aged one year and eight months, had been suffering for three weeks with a very irritable state of the bowels, so much so that it could not be kept clean; and during an evacuation a round worm was passed. The child looked pale and ill; it had been fed with the ordinary diet of the house and pork. A mixture of a grain of santonine and syrup was ordered to be taken three times a day. On the next visit (four days), the child had voided forty-six round worms. It seemed more cheerful, and was to continue the mixture. At the next visit, fifteen more had passed, making in all sixty-two worms.—*British Medical Journal*, May 30, 1874, p. 707.

37.—ON THE ACTION OF PURGATIVE MEDICINES.

By Dr. T. LAUDER BRUNTON, Casualty Physician and Lecturer on Materia Medica at St. Bartholomew's Hospital.

[A matter of great importance with reference to this subject is the effect of purgatives upon the secretions which are poured into the intestinal tube by the various glands which are connected with it.]

The saliva which flows into the mouth from the submaxillary and parotid glands is swallowed and aids the digestion of starchy food in the stomach, and probably the intestine. A part of its active principle, ptyalin, is reabsorbed, and some of it is excreted in the urine; but as we shall here afterwards see, it is probable that another part is excreted again by the salivary glands and thus does its work twice over. This is at present only a probability as regards ptyalin, but it is a certainty in the case of several substances which are excreted by the salivary glands, such as iodide of potassium, for example, which can be detected with great ease. When this substance is swallowed, it is absorbed from the stomach, passes in the blood to the salivary glands, and is excreted by them much

more readily than by the kidneys. It again passes down with the saliva to the stomach, is reabsorbed, and again excreted. Thus it may go round and round for a long time without getting entirely out of the body. If we wish to remove it quickly and completely, we must give a purgative so as to prevent its reabsorption from the intestinal canal by causing its speedy expulsion. The same is the case with other iodides, such as those of lead or iron. Iodine has been shown by Bernard to possess the power of making iron pass readily through the salivary glands, the iodide of iron being found in the saliva soon after it has been injected into the blood, while other salts of iron, such as lactate, never make their appearance in it at all. Several years ago iodide of potassium was proposed by MM. Guillot and Melsens as a remedy in cases of lead-poisoning. The lead, they consider, is present in the body in the form of an insoluble compound which it makes with the tissues, but by the administration of iodide of potassium it is rendered soluble. It then finds its way into the circulation, and is excreted by the kidneys and other emunctories. But the iodide of lead is partly excreted by the salivary glands, for M. Malherbe, of Nantes, and Dr. Sieveking have found it in the saliva of persons suffering from lead-poisoning, and who were being treated by iodide of potassium. The lead salt being swallowed with the saliva, is again reabsorbed, and thus the cure is comparatively slow when patients are treated with iodide of potassium alone. I frequently see patients suffering from lead-poisoning brought on by working in white lead, and for some time I have been accustomed to treat them with five grains of iodide of potassium, three times a day, and a sufficient quantity of sulphate of magnesia or other purgative either thrice or once a day, to keep the bowels very freely open, and cause the expulsion of the lead from the alimentary canal as quickly as it is secreted into it. I have not made comparative experiments on the effect of this treatment and of that by iodide of potassium alone, or by purgatives alone, but from what I remember of cases treated by the late Professor Syme with castor-oil, I am fully satisfied with the treatment I now adopt. The same plan would probably prove equally useful in chronic poisoning by copper or mercury.

But the gastro-salivary circle, as we may term it, from stomach to salivary glands and from salivary glands to stomach again, is not the only one in which those metals move. Their circulation in the portal system, or entero-hepatic, as it is termed by Lussana, is still more important. Iron is eliminated in great part by the bile: copper and manganese appear in it also, according to Albin and Moser, and it seems probable that manganese, lead, and all the heavy metals pass out of the

body by this channel. From the liver they pass into the intestine, are reabsorbed from it, and again pass to the liver and recommence their course. They may be present in considerable quantities in the blood of the portal system without reaching the general circulation or getting a chance of passing out in the urine. They are therefore much more closely locked up in the entero-hepatic circulation than in the gastro-salivary one, for the salivary glands are supplied by the systemic circulation, and any blood which brings lead or any other substance to them must also carry it to the kidneys. The power of the entero-hepatic circulation to retain metals within the body being much greater than that of the gastro-salivary one, it is evident that the beneficial effects of purgatives in lead-poisoning are due to their removing the metal from the portal circulation still more than their action on the gastro-salivary one which has already been discussed. Other poisons, such as curare and probably serpent's venom, may also circulate in considerable quantity in the portal system without reaching the systemic circulation, and probably this is one of the causes, though by no means the only one, which renders these substances to a great extent innocuous when swallowed.

But the circulation of iron, lead, curare, &c., in the portal system, important though it may be, is of far less interest than the circulation of the bile itself. For the sake of convenience I have merely stated that lead, mercury, &c., are excreted in the bile, and have hitherto assumed that bile circulates in a similar way in the portal system, without giving any reason for doing so.

It used to be thought by many that bile was formed in various parts of the body, and was simply excreted by the liver. This view is now given up by most physiologists, who believe that bile is formed by the liver only. But in altering their views regarding the function of this organ they went too far, and supposed that it only formed bile, which, when it had once found its way into the intestine and mixed with the intestinal contents, became decomposed and finally expelled with the fæces. A year or two ago, however, Schiff found that this view of the hepatic functions was too limited, and that the liver removed bile from the blood or *excreted* it as well as formed or *secreted* it. He observed that when all the bile was drawn away from the liver by means of a fistulous opening in the gall-badder after ligature of the ductus choledochus, the quantity which flowed from the liver rapidly diminished after the fistula had been established, but could again be quickly increased by the simple process of putting bile into the duodenum. The bile was at once absorbed and again excreted by the liver, and it did not make much

difference whether the bile just removed from the fistula in a dog was again injected into its duodenum, or whether ox bile was used instead. In the normal state of the animal the liver is always doing two things: it is *forming* new bile, and it is *excreting* old bile which it has received from the intestine by means of the portal vessels. When a biliary fistula is made and the bile is drawn away as fast as it is secreted, none gets into the intestine, and therefore no old bile reaches the liver; consequently, the quantity collected represents only the new bile formed in the liver, and is of course much less than that which would normally pass through the ductus choledochus into the intestine. If all the bile were absorbed there would be no need for the liver to go on forming it, but this is not the case, for only a part of it is reabsorbed, and the remainder is decomposed and excreted with the fæces.

So long as the liver does its duty properly, and excretes again all the bile which is absorbed by the portal blood from the intestine, very little bile can pass through the organ into the vena cava and thence into the general circulation. But whenever so much bile is taken up from the intestines that the liver cannot excrete it all, it will find its way out of the portal into the systemic circulation, and will exert an injurious action on the nervous system. The same effect will follow anything which diminishes the excreting power of the liver and renders it unable to excrete the normal amount. It is evident that if anything should cause the liver to form more bile than usual at any time, it will have extra work to do in the way of excreting it after its absorption, and there will be more bile circulating in the portal blood for some time afterwards, or at any rate until the extra quantity has been got rid of or compensation has been established by the liver forming less. Many experiments have shown that an abundant supply of food causes the liver to form more bile, and we all know that heavy dinners are apt to cause biliousness. Fasting, on the other hand, diminishes the quantity of bile secreted, and everyone knows that if he fasts for a day after taking an especially heavy dinner he may be none the worse for it, but if he dines out every night he is almost sure to become bilious unless he takes measures to prevent it by using purgatives.

It has not yet been shown by direct experiment that the symptoms usually grouped under the head of "biliousness" are due to the presence of an excess of bile in the blood; but the rapidity with which they disappear after the removal of bile from the system, either by vomiting or purgation, renders it extremely probable. Frequently we find that the fit of vomiting which has expelled a quantity of bile is hardly over

when the appetite returns, the brownish-white fur disappears from the tongue, the face loses its dingy hue, the languor disappears, the irritability of temper is replaced by equanimity, and stupidity and laziness give place to sprightliness and activity. But vomiting is a disagreeable process, and few submit willingly to it, although it would be well worth while if the same ends could be gained by no other means. As most old practitioners have found, however, a mercurial pill and a saline purgative produce all the good effects of vomiting without its trouble and discomfort, and they have long been in the habit of ascribing the beneficial action of the mercury to its "cholagogue" properties. They felt convinced that biliousness was due to bile in the blood, and believed that its removal was due to the liver being stimulated by the mercury to excrete the bile more rapidly. But the careful experiments made by the Edinburgh Committee of the British Association on dogs with biliary fistula showed that neither mercurials nor other purgatives increased the flow of bile from the liver, and these results seemed at first sight to contradict the views entertained by most practitioners regarding their cholagogue action. The contradiction is apparent, but not real, for in the experiments the bile was regularly removed from the body as soon as it was formed, and none of it ever reached the intestine. Consequently, any diminution in the quantity collected simply showed that the liver was forming less. Other experiments have given somewhat different results from those of the Edinburgh Committee, and Röhrig has found that the administration of purgatives, as well as other measures which increase the circulation in the portal system, augment the formation of bile. The important question in regard to the treatment of biliousness, however, is not whether the liver forms more or less new bile, but whether the bile already circulating in the blood is removed from it. The liver may be doing its best to effect this purpose, but it will not succeed if the bile it removes from the portal blood is again absorbed as quickly as it is poured into the intestine. But if the peristaltic action of the whole intestinal canal is quickened by a purgative, the bile will be hurried rapidly onwards and evacuated before there has been time for its reabsorption, and the liver being thus relieved will be able to excrete any bile still remaining in the blood. This result will not be effected by any purgative acting on the large intestine alone, for a considerable part of the bile will in all probability have been absorbed before it gets so far; but any simple purgative or mixture of purgatives which stimulates the duodenum and small intestine as well as the large one will prove most effectual. Now, the green colour which the fæces present after the administration of mercurials, and which is so distinctive

that the name of "calomel stools" has been applied to them, has long been regarded as an evidence of bile and appealed to as a proof of the cholagogue action of these remedies. The opponents of this doctrine have declared that the colour was simply caused by the presence of black sulphide of mercury, just as a somewhat similar colour may be occasioned by the presence of a small quantity of sulphide of iron after the administration of mild ferruginous preparations. Their statement has been disproved by Buchheim, who has shown that the colour is really due to bile, and thus established the fact that calomel induces its expulsion from the intestine. It may therefore well be called a cholagogue, and it is evident from what has already been said that it must diminish the quantity circulating in the blood, whatever its effect may be on the amount formed by the liver.

[The following is a short *resumé* of the chief points in this paper.]

Purgatives act by stimulating the secretion of fluid from the intestines, as well as by increasing peristaltic action. They prove useful in many ways. They hurry the food out of the alimentary canal, and thus lessen the injurious effects of over-eating. By expelling irritating substances from the intestine they arrest diarrhoea, and remove headache and other pains, caused either by the abdominal irritation or by the absorption of poisonous matters produced by imperfect digestion and decomposition of food. They relieve biliousness by removing bile, and are most efficient aids in the treatment of chronic poisoning by lead, mercury, or other metals. It is probable that pepsin and pancreatic ferment are absorbed from the intestine and circulate in the blood, where the latter assists in the production of animal heat. They are then secreted anew by the stomach and pancreas, and do their work again. Purgatives lessen their quantity as well as that of the bile; they may thus be useful in fevers, but they injure old and feeble persons, both by diminishing their calorific power and impairing their digestion. They relieve inflammation by lowering the blood-pressure and thus diminishing congestion; and they prove beneficial in dropsies, both by abstracting water from the blood and diminishing congestion in the kidneys.—*Practitioner*, June 1874, p. 403.

DISEASES OF THE URINARY ORGANS.

38.—ON THE ESTIMATION OF THE NITROGENOUS CONTENTS OF URINE.

By Professor EMERSON REYNOLDS.

In examining urine it is generally considered sufficient to estimate the highly nitrogenised urea and uric acid in order to determine the nitrogen excreted in a given time, since these bodies are commonly regarded as the principal product of the waste of nitrogenised tissue. Such a mode of measuring the nitrogen excreted by the kidneys in health is probably near enough to the truth for practical purposes; but we have good reason to believe that it is not by any means a true measure under diseased conditions; moreover, it is imperfect, because the existing method of estimating uric acid is, in my experience, quite untrustworthy, though error in this direction is perhaps of little moment when the proportion of the acid present is very small. In view of these considerations I have long felt that the *total* nitrogen in urine must be directly determined if we desire to obtain a true measure of its excretion by the kidneys under diseased conditions of the body. Assuming that we find, with the aid of Liebig's well-known volumetric process, the proportion of urea present in a specimen of urine, we can easily calculate the amount of nitrogen present in that urea. If, now we possess the means of estimating the *total nitrogen* in the urine, it is clear that we have only to subtract from this quantity the nitrogen present as urea in order to find the amount contained in uric acid, creatine, creatinine, hippuric acid, or other nitrogenised bodies which may happen to be present as morbid products. In my judgment, far too little attention has hitherto been given to the determination of this *residual nitrogen*, if I may be allowed the term, in diseased conditions. This may be due to the fact that under ordinary circumstances the determination of total nitrogen can only be successfully made by a person possessing some skill in one of the methods of organic analysis; but I have now the pleasure to describe very briefly a process which yields the desired results and yet is exceedingly simple and expeditious. I take a small tube of hard glass, about five inches long and three-eighths of an inch internal diameter; one end of this is drawn out before the blowpipe and bent so as to form an angle of about 45°. The tube is now supported in an inclined position and one cubic centimetre of the urine (about sixteen minims in bulk) very accurately measured into it by means of a carefully graduated pipette. A drop of sulphuric acid is then added and the liquid

evaporated in the tube at a gentle heat, a current of air produced by an aspirator being made to pass over the surface of the liquid. When this small quantity of urine has evaporated nearly to dryness, the fine end of the tube is hermetically sealed, a quantity of "soda-lime" is then introduced, and the mouth closed by a good cork carrying a V-tube containing a very little diluted hydrochloric acid. A strong heat is now applied to the mixture of urine residue and soda-lime, and the ammonia resulting from the decomposition of the nitrogenised organic matters driven off. All the nitrogen present in the cubic centimetre of urine is thus obtained in the form of ammonia, the latter being wholly condensed in the acid contained in the V-tube. At the termination of the operation the acid is washed out of the V-tube into a glass cylinder, neutralised with pure potash diluted up to a known bulk, and then treated with "Nessler solution," the well-known and extremely delicate colour-test for ammonia. The depth of the brown tint produced by the reagent measures the proportion of ammonia, and therefore of total nitrogen, in the quantity of urine taken for experiment. The value of the result of the colour test is very expeditiously obtained by having at hand a number of standard solutions of varying depths of tint for comparison. If the total bulk of urine excreted in a given time be known we can thus easily and quickly determine with very exact precision the quantity of nitrogen removed by the kidneys.—*Medical Press and Circular*, May 13, 1874, p. 402.

39.—THE ACTION OF VALERIAN IN DIABETES MELLITUS.

M. Bouchard has lately investigated the influence of valerian on diabetes insipidus, and finds that it acts by diminishing the excretion of urea, and so secondarily the polyuria. After eight grammes of the extract of valerian the urea excreted may fall to forty grammes, or about 600 grains, per diem; and there is no diminution in the amount of urine observed until the urea has fallen below its normal quantity, and then only 2000 or 1500 grammes—*i.e.*, four or three pints—may be excreted in twenty-four hours. In other diseases the action of valerian is variable, but where any exists it can always be referred to the diminution in excretion of urea which it produces; and if there is no azoturia the action is *nil*. Thus, in diabetes mellitus, if urea is in excess, it may fall while the patient takes valerian from forty-five grammes to twenty-five or even nineteen per diem, and then the polyuria and glycosuria diminish in their turn. Dr. Bouchard considers that the action of valerian is to spare waste of tissue, and in support of this

view he mentions that certain Indians of Lower California and Mexico are accustomed to go through a course of it for a month before they enter upon a severe expedition, so that they may be better able to bear fatigue. He gives it in frequent small doses, and gradually increases the amount taken until he has in some cases reached a dose of thirty grammes in twenty-four hours.—*Medical Times and Gazette*, May 23, 1874, p. 568.

40.—ON DIABETES FROM CARBONIC OXIDE.

By Dr. B. W. RICHARDSON, F.R.S.

The brief notice of Dr. Pavy's interesting experiments on the production of diabetes by the inhalation of the fumes of the *Lycoperdon giganteum* (common puff-ball), reported in the *Lancet* of last week, calls from me the following brief notice.

Immediately after I had discovered the anæsthetic action of the fumes of burning lycoperdon—viz., in 1853,—research was made as to the agent in the fumes that produced the physiological effect. I commenced this inquiry on my own account; but before my analysis was completed I received a request from Snow to visit him at his laboratory in Sackville-street to witness his analysis. I went as desired, and was shown, beyond all dispute, by Snow, that the active agent in puff-ball smoke is carbonic oxide gas. A short time afterwards, and before Snow had time to publish his new observation, Thornton Herapath sent me word that he had made the same discovery. Herapath published an account of his research at once, so that the originality of the observation that carbonic oxide is a product of the slow combustion of the lycoperdon has very justly rested with him.

When the matter under inquiry had been demonstrated thus far, I commenced a series of researches on the comparative action of carbonic oxide upon animals. I found precisely the same effects from this gas when it was administered, well diluted with air, as were observed from the fumes of the lycoperdon. The insensibility produced was the same, and the peculiar bright redness of the arterial blood was the same.

At the same time a most interesting new point was observed—namely, that dogs made to inhale carbonic oxide were rendered temporarily diabetic. I collected the urine passed after exposure to the gas, and found it contained glucose in considerable quantity. I made this subject a point of illustration in a lecture on diabetes delivered at the Grosvenor-place School of Medicine on November 22nd, 1861; and the report of the lecture, published by Mr. Macpherson in the *Medical Times*

and Gazette of March 8th, 1862, although it is merely an abstract of the lecture, contains a brief but correct notice of the fact named. In an after experiment I found that coal-gas, which contains carbonic oxide, produces the same conditions as diluted carbonic oxide—namely, redness of the venous blood and saccharine urine.

A corroborative and singularly interesting observation has been made later still by Dr. Hesse. He noticed that two men who had been exposed, by accident, to the fumes arising from a stove heated by pit-coal, when they had recovered from the more imminent symptoms, were suffering from diabetes; their urine was charged with diabetic sugar.

In my experiments on the inhalation of oxygen, which have been extensive, I have compared the action of oxygen with that of carbonic oxide. In respect to the urine, it has seemed to me that there is a difference, and that sugar can only be produced freely by the action of carbonic oxide. A prolonged inhalation of an atmosphere in which carbonic acid was present in such weak dilution with common air as to cause torpor without asphyxia caused, however, evidence of sugar.

The hypothesis I formed, when I first observed the action of carbonic oxide in producing saccharine urine, was that the gas exerted its toxic influence through the nervous system. I doubt now whether this view is correct. In the last few years the researches that have been published on carbonic oxide, especially by Dr. Arthur Gamgee in his splendid paper published in the *Journal of Anatomy and Physiology* for May, 1867, point, I think, to the possible conclusion that the effect of the gas may be chemical and direct in the production of glucose in the animal secretion.

In Dr. Pavy's experiments with the fumes of the *Lycoperdon giganteum*, the agent at work has certainly not been oxygen, but carbonic oxide; and as my experiments have proved that the inhalation of carbonic oxide will produce artificial diabetes, and as it has been proved from the experiments of Bernard, Meyer, Hoppe, and Gamgee that carbonic oxide has the power of displacing the oxygen of the blood, I infer that when sugar is formed by the synthetical method now being considered, carbonic oxide, and not oxygen, is, either directly or indirectly, the cause of the phenomenon.—*Lancet*, Sept. 5, 1874, p. 340.

41.—ON THE THERAPEUTIC VALUE OF SOFT WATER IN URINARY CALCULI AND INDIGESTION.

By Dr. JOHN C. MURRAY, Newcastle-on-Tyne.

For seventeen years that I perseveringly attempted the solution of stone with hard water, I experienced the same disap-

pointment which, in the practice of physicians in London and other hard-water areas, has doubtless prevented chemical solvents from being of service, the quantity of mineral acid which would agree with the stomach being often just sufficient to neutralise the earthy salts daily consumed in the water, or at least not adequate to check the tendency to alkaluria. I have frequently, indeed, thought they did harm by causing some of the lime and magnesia to enter the blood in solution, which else might have passed through the bowels. Alkalies also frequently increase the irritation of the stomach, and cause a greater formation of acid, as explained by Professor Trouseau. For the last thirty months I have ordered every bilious or calculous patient to use rain or distilled water, and the result has been so successful that I have a respectful fear of detailing the cases from dread of scepticism, and am therefore the more anxious that the medical treatment of calculi should be undertaken by one or more disinterested physicians, as I have suggested, in the wards of a hospital, so that the cases may be watched by the profession. I may, however, state that, as the result of this practice, I have a nice little collection of water-worn calculi, and that I have been disappointed of many more, some having been passed while bathing (as attested by the sharp forcing pain, and lacerated, bleeding urethra), some in the water-closet or by the wayside, a very interesting case by crumbling to powder on a warm mantelpiece, and not a few by patients at a distance neglecting their own interests and that of science by not forwarding them.

Even those who are unbelievers in the reduction of calculi by means of solvents are in no slight degree bound to support a crucial test as combined with distilled water—first, in the interests of that too numerous class who have renal calculi; second, as operation for stone has to be repeated once in about six times, that the surgeon may tell the patient how to prevent a recurrence; third, when lithotrity is performed to assist in washing away the detritus.

There ought to be no lack of interest in this subject, for during the past year its importance has been emphasised by the fact that an ounce of lime has put *hors de vie* one of the most marked men in the world's history. Permit me to glance at the late Emperor Napoleon's case as one in point. The frequent and heroic courses of Vichy water and other alkaline waters which the late Emperor took, probably under the advice or impression that the pains which he frequently suffered were of rheumatic, and not, as they seem to have been, of dyspeptic and calculous origin, would have a chemical tendency to cause the deposit which formed the nucleus of his calculus—viz., amorphous phosphate of lime,—and surround it by the

crystalline phosphate. When he came to Chislehurst, the chalk water there, together with the irritation of the slowly forming stone, would be sufficient to decompose urea and cause rapid deposit of the triple phosphate, lime and mucus having especial tendency to decompose urea. Had the Emperor gone to reside in a part where the water was soft, such as where the Empress's Scottish ancestors belonged, or used rain or distilled water with mineral acids, I think he might have lived, and possibly once more have sat upon the throne of France, and that the calculus would have slowly decreased in size, becoming at last sufficiently small to pass *per via naturales*, as the post-mortem appearances seemed to indicate that many previous ones had done.

To prescribe soft or distilled water in cases of stone or gravel is somewhat new in medicine, and to order it in indigestion and derangement of the liver is still more novel, at least out of London, although the mode of life may be accumulating in the system the elements of biliary or renal calculi which are being eliminated with daily increasing difficulty; yet the best time for treatment is obviously before the stomach, liver, and kidneys become seriously impaired. The great benefit which is often derived from a change of residence in stomach and liver complaints is probably from change of water rather than of air. Air being so diffusible, it cannot be much different in one part of the country from another, but the water is not alike in any two places. I therefore hope that before long we shall have a tabulated statement of the qualities of the drinking-water in the various health resorts, so that we may know where to send hepatic and calculous patients for their advantage. At present there is too much haphazard in the matter of recommending a change. Generally, if a place has fewer wet days, a quarter per cent. less carbonic acid, or a suspicion of more ozone than another, it is preferred, without taking into account that from the five pounds of water daily used the patient may take fever and die, or contract the nucleus of a calculus and linger only to suffer a life of misery.

Few are more sensitive than I am to the impropriety of raising unnecessary alarms, for I firmly believe it an excellent rule (preventive medicine notwithstanding) to eschew medicine unless when really ill. To this wholesome rule, however, cases of stone form a frequent exception. A child, adolescent, or woman, for example, may have a stone gradually enlarging without being aware of it; whereas a man of forty or upwards, being cognisant of liability, is alive, if not to the first, at least to early symptoms, and generally seeks advice.

Deeming that the prevention and solvent treatment of calculi demand more careful and extended practical attention

than the subject has yet received, I beg respectfully to press upon the profession the great importance of this line of study, trusting that the authors of our future text-books on urinary pathology will devote more time and space to this too neglected department of our art; a sub-division, moreover, which yearly becomes of more consequence as we further depart from rural habits and live more at the call of the telegraph and upon the rail. For railway travelling, it has been observed, tends to induce urinary disease.

But if the circumstances of our being be different from those of our ancestors, we are also possessed of more chemical and physiological knowledge calculated to counteract the evil effects of dense population and trade competition; and if we do not apply that knowledge at least as soon as the need for it becomes apparent, we would deserve to suffer the punishment, provided always that it fell only upon the guilty. Our mode of living in 1874 does not permit all of us to eat and drink what we might choose, but too often only when and what is most convenient, or will best suit us. If our system gets out of order, it is an indication that we are living in error; and the chemistry of this year of grace would deserve opprobrium were it incapable of showing in what that error consisted. I fear, however, that the fault does not lie with chemistry, but with medicine. Our bodies are chemical laboratories, and every physician, especially those who lead in urology, ought to be expert chemists in order to remove our practice from the field of empiricism, and render it sufficiently plain for those who, in consequence of lacking time for study—country practitioners, for example,—may be satisfied with the opinions of others, and in a position to treat their trusting patients in a rational manner. Trousseau, in his lectures at the Hôtel-Dieu, said that 299 physicians in 300 were very bad chemists, and immediately after gave a sufficient reason for disregarding chemistry in medicine by stating that “therapeutical action does not admit of chemical explanation: it is essentially vital or physiological.” We must be upon our guard against this convenient way of shelving our chemical books. I confess to being unfortunately one of the 299 who have forgotten most of my chemistry, and therefore cannot speak oracularly upon chemical decompositions within the body, but the principle, being supported in practice, must be essentially right; and be it observed, moreover, that animal chemists are not content with a theory unless they can produce the same compounds in the laboratory that are formed in the organism. Urine and all the fluids of the body have chemical reactions, and however much vital or physiological action, or albumen taking the place of an acid, may accelerate or retard chemical com-

binations and decompositions, they will, nevertheless, take place. It was proved by Brande in 1808 that urine can be rendered acid or alkaline by medicine, and being only a secretion from a vital fluid, and not itself vital, chemistry, in regard to it at least, is paramount over "vital action;" hence the sooner and more diligently we return to our chemical studies in urinary complaints the better.

There is an imminent risk of calculus where the urine shows frequent deposits; if they consist of uric acid or urates, it indicates (1) that the blood is insufficiently alkaline, (2) that the urine is too acid, and (3) that there is a deficiency of water. We should then give half-drachm to one-drachm doses of bicarbonate, citrate, or acetate of potash four times daily with twenty ounces of soft water, and direct that everything which tends to form or cause deposition of uric acid or urates be avoided, such as much animal food, fat, pastry, sugar, acid fruits, over-eating, alcohol in any form, sulphur (or articles rich in it, as peas, beans, eggs, cabbage, mustard); also that the patient take more exercise in order to oxidise the uric acid and to encourage perspiration and consequent elimination by the skin of carbonic, uric, butyric, and lactic acids. When oxalate of lime and phosphates are precipitated as gravel, the urine is generally plentiful, and opposite directions are, as a rule, appropriate. For example, the more animal food which is eaten the better; laziness is an advantage; a little whisky or brandy is admissible; mineral acids must be taken; and a lesser quantity of soft water will suffice. I have not found nitro-muriatic acid fail in restoring the urine to an acid state. Even in ammonæmia with subacute cystitis and phosphatic renal calculi, it has sufficed, when the food was prepared with distilled water, and also about three pints drank in twenty-four hours so as to maintain diuresis.

The symptoms of stone having already formed are generally so evident that sounding, even when applicable, is unnecessary, except to ascertain its size. The best indications for treatment are then, as in all urinary complaints, afforded by the frequent use of the microscope, for different layers in a calculus often require diverse treatment. But while studying appropriate remedies the dissolving process may still be going on, for "water will hard marble wear away," and copious draughts of soft or distilled water will have the same effects upon a stone. Even without medicine it will produce an impression upon a calculus of any kind, and will preserve in solution and wash away the material which would otherwise accumulate upon it; and, what is very important, it will improve the health. Soft water, even a deluge of it, is rarely, if ever, otherwise than beneficial. It dissolves the nutritive

matter out of the food, thus supplying the body with aliment which would otherwise pass through the bowels undissolved or enter the circulation in a state less capable of healthy assimilation. The advantage which is derived from the free use of soft or distilled water in improved elasticity of frame and diminution of fat in the obese is a recommendation in favour of diluents. If the nuclei of calculi were examined when recent it would probably be found that microscopic crystals of oxalate of lime formed the nucleoli of 20 per cent., uric acid 70 per cent., and biurate of lime and biurate of magnesia nearly the remaining 10 per cent. If the urine were preserved in an unsaturated state by sufficient soft water, no nucleus would form, or if a deposit did occur upon a particle of mucus, it would be washed away by the frequent renewal of the urine passing over it.

I have said nothing relative to the solution of stone by injection with or without galvanism, because I think that the less the bladder and prostate are irritated by instruments the better, for even the passing of a catheter, as in Baron Dupin's case, may cause death. But where a triple phosphate or oxalate of lime calculus is causing serious mucous irritation, the bladder might advantageously be subjected to a four hours' irrigation with nitro-muriatic acid in distilled water at blood heat, in order to begin the treatment, and check the formation of mucus, pus, and the decomposition of urea.

I am much pleased to find that since I first penned this paper, in January, 1873, Dr. Jno. Gardner, of London, has published an interesting volume on "Longevity," in which he recommends the free use of distilled water for preventing disease and prolonging life; and that Mr. Cadge has within the last few weeks given to the profession recent and extensive statistics, which will be of great service in this line of study.

I firmly believe that we may reply affirmatively to Sir Henry Thompson's question in "The Preventive Treatment of Calculous Disease"—"Can we do anything to prevent the formation of stone in the urinary passages?" and think with Dr. Hassall "that the very circumstance of the occurrence of stone affords a convincing proof of the incompleteness of the present state of medical science;" and trust with Dr. Harley "that the day is not far distant when a patient with a stone of any size in his bladder will be looked upon in the light of a clinical curiosity."

The object of this paper is to keep the solvent mode of treatment before the profession, in order that that day may be brought more near. — *Medical Times and Gazette*, Oct. 24, 1874, p. 469.

SURGERY.

FRACTURES, DISLOCATIONS, AMPUTATIONS, & DISEASES
OF BONES, JOINTS, ETC.

42.—ON AN IMPROVED EXTENSION APPARATUS FOR THE TREATMENT OF FRACTURES OF THE SHAFT OF THE FEMUR.

By G. BUCKSTON BROWNE, Esq., Demonstrator of Anatomy
at University College, London.

The perfect cure of an oblique fracture of the shaft of the femur, when the ends of the bone have been completely displaced, has baffled the ingenuity and the skill of surgeons from the time of Hippocrates. The fact that a recovery with half or three-quarters of an inch shortening is at present considered satisfactory should serve as an excuse for the introduction of any improvement in treatment, however slight the alteration may appear.

The opinion of surgeons at the present day will be found to be almost unanimously in favour of treatment in the straight position, as ordinarily practised before Percivall Pott advocated the so-called physiological method or treatment by position.

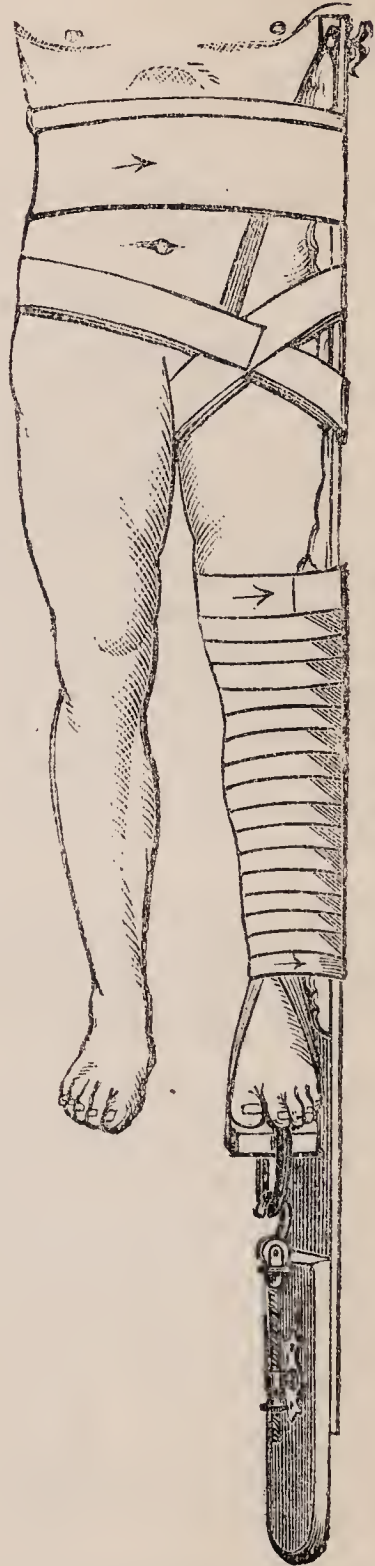
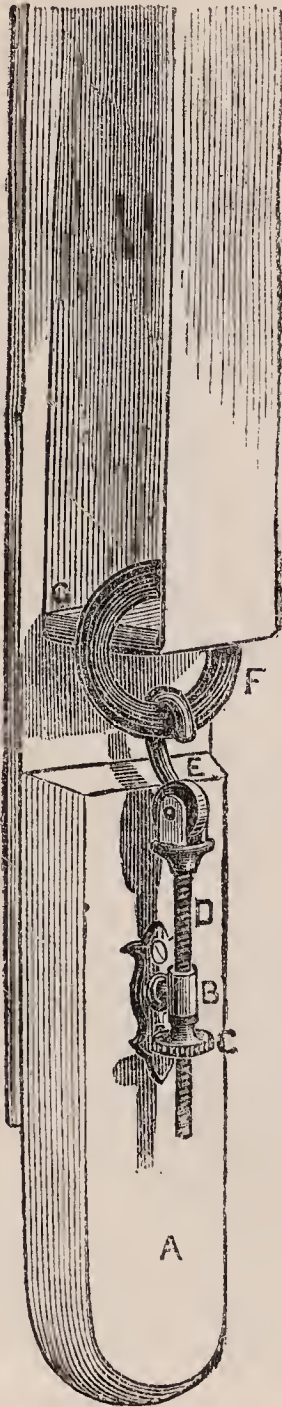
Hitherto the difficulty has been the proper application of extension. Ordinarily it has been kept up by means of a long splint (Liston's splint), or by means of weights or screws, or, more recently, by elastic traction. The objections to Liston's long splint are that it is apt to strain the ankle-joint, and that, notwithstanding the greatest care, awkward sores are liable to form about the malleoli and over the tendo Achillis. Serious shortening, moreover, not unfrequently attends this mode of treatment. Weight extension, however carefully applied, is generally cumbrous, and always more or less remittent, for the cord is very apt to get entangled with the bed-clothes, or to slip off its pulley, especially when the patient has to be moved.

Mr. De Morgan and Mr. Cripps have each invented ingenious splints, in which continuous elastic traction is employed. It

was in search of an apparatus rather simpler than these that the writer first employed the elastic stirrup extension, described

FIG. 1.

FIG. 2.



in Mr. Berkeley Hill's "Essentials of Bandaging," p. 74. It was found, however, that the extension could not be properly

regulated, and that the foot was drawn so forcibly outwards towards the splint that much force was lost, and the skin over the external malleolus endangered. To remove these objections the apparatus here figured was proposed, and, meeting with Mr. Erichsen's approval, was adopted in his wards, with hitherto good results.

The figures will for the most part explain themselves. The foot-piece (G, Fig. 1) is a piece of wood, four inches by two and a half, notched to receive a stout india-rubber ring (F), technically known as a "horse's cutting ring." The foot-piece and the ring are secured to the limb by means of an ordinary stirrup of stout strapping, two inches wide, extending above the knee, so as to relieve the joint of strain. A long splint is prepared, reaching from the axilla to about eight inches beyond the foot, to the lower four inches of which is screwed a piece of one-inch plank (A), about eight inches long. This piece of wood throws the direction of the traction into a line with the axis of the limb. The limb is then bandaged in the usual way to the long splint, the bandaging being begun above the ankle. When the bandage has been carried above the knee, the leg and splint are grasped together by the surgeon, and extension is made while the perineal band is tightened. A hip spica and a body roller complete the bandaging, but it is well to secure the body roller to the splint by a row of tinned tacks. (In the figure the bandaging is left imperfect to show the perineal band.) Then on the plank (A) is screwed the bracket (B), through which slides the stem (D) of the hook (E). Along the stem and below the bracket runs the screw-nut (C). The hook is now fixed to the ring; and, the stem being passed through the bracket, extension can be made by screwing up the nut. The bracket and stem are similar to those sold by ironmongers for securing the cords of window blinds, except that in place of the hook there is a little grooved wheel. This wheel is easily changed for a hook; or the whole apparatus, ring and hook, can be obtained from Messrs. Mayer and Meltzer, of Great Portland Street.

The advantages of this instrument may be thus summed up:—The amount of extension can be regulated to a nicety; the extension is constant, for it remains unaltered when the patient is moved or lifted out of bed; and the bony prominences of the foot are free from all pressure. Finally, it is hoped that by the adoption of this very cheap and simple method shortening may be reduced to a minimum.

The following results have been obtained by the writer, and by his successor as house-surgeon, Mr. Maclean, in five con-

secutive cases of oblique fracture of the shaft. All, except the second, were caused by indirect violence, the last two being very severe cases of double fracture:—

Name	Sex.	Age.	Seat of Fracture.	Result.
A. M. ...	Male ...	28 ...	In lower third	No shortening.
J. B. ...	„ ...	17 ...	In middle third	„
J. C. ...	„ ...	65 ...	{ Junction of upper and middle third }	„
J. N. ...	„ ...	37 ..	{ Double fracture: 1. Lower third (oblique) 2. A fracture into the knee-joint }	... $\frac{1}{2}$ -in. shortening.
A. W. ...	„ ...	8 ...	{ Double fracture: 1. Junction of upper and middle third. ... 2. Junction of middle and lower third. }	... $\frac{1}{3}$ -in. shortening.

Many other cases of fracture of the femur which have been treated in this way are not mentioned, as they do not come under the head of oblique fracture of the shaft with shortening and deformity.—*Lancet*, Oct. 10, 1874, p. 511.

43.—EXCISION OF THE ANTERIOR TARSUS AND BASE OF THE METATARSUS—A NEW OPERATION.

By Dr. PATRICK HERON WATSON, F.R.S., Senior Surgeon to the Edinburgh Royal Infirmary.

[The patient was a lad, æt. 19, suffering from disease of the anterior portion of the tarsus, spontaneous in its origin, sub-acute in its progress, and productive of such pain on movement or pressure that the lad was unable to walk or work. Amputation of the foot seemed too severe a measure to be justified by the condition of the parts, although there could be no reasonable doubt that suppuration had already commenced within the bones and joints involved. After reviewing Chopart's, Hey's, and other possible operations, and showing their unsuitability for this case, the writer continues as follows.]

The operation I will now describe was justified as a proceeding amply sufficient to effect the extirpation of all actually existing diseases of bones and joints in the foot, and of securing as a result a foot which would be more seemly, if not more useful, than an amputation at the ankle-joint. After the application of the tourniquet to the lower part of the thigh, the operation was effected by making an incision on the outer and inner sides of the foot, between three and four inches in length; that upon the outer side extending from the centre of the outer margin of the plantar surface of the os calcis as

far as the middle of the metatarsal bone of the little toe, that upon the inner side of the foot from the neck of the astragalus to the middle of the metatarsal bone of the great toe. The soft parts were then carefully dissected off the dorsal and plantar surfaces of the tarsus from the outer and inner sides, until the whole extent of osseous tissues to be removed was deprived of its soft coverings. In effecting this, the thumb of the left hand formed the guide to the point and edge of the knife in keeping close to the surface of the bones, so as to avoid any interference with the important structures contained in the soft parts. A curved probe-pointed bistoury inserted between the soft parts and bones was then carried across the line of articulation, between the astragalus and scaphoid and os calcis and cuboid bones, first upon the dorsal, and then upon the plantar surface, so as to open up these joints. A little further use of the knife completed the severance of the bones posteriorly. A keyhole-saw was now introduced between the plantar soft parts and the shafts of the metatarsal bones, which were then cut through, one handle of a pair of bone forceps being inserted between the metatarsal bones and dorsal soft parts to protect the latter from injury by the teeth of the saw cutting from below upwards.

After the operation, and before removing the tourniquet, the entire wound was plugged firmly and securely by means of pledgets of lint passed through and through the gap in the foot, and laid in closely one upon the back of the other, so as completely to fill up the aperture, and, at the same time, so closely crowded together as to secure, when pressure was applied from the outside by other pads supported by a bandage, that no bleeding should take place. This dressing was retained *in situ* for forty-eight hours. When removed there was no bleeding, and the dressing was reapplied. The parts were dressed from day to day, pads of lint being placed in the cavity, with the view both of affording internal support to the soft parts, and also of securing that consolidation should take place from all the surfaces of the wound equally, without the possibility of an early closure of the lines of incision giving rise to bagging of matter and after disturbance. Externally, support was effected by two gutta-percha splints applied upon each side of the foot, external to the dressings, so as to admit of the patient getting out of bed and moving about with crutches at as early a period as possible.

The dressing was inserted into the wound for a period of six weeks, after which it was only applied superficially; the openings had not, however, closed finally for three months. During the latter portion of this period, a gutta-percha pad was bandaged against the hollow of the foot, so as artificially to

maintain an arch between the posterior part of the foot and the anterior extremity of the metatarsus.

When I last saw this patient, six months after the operation, he walked smartly and well, with none of the stumping gait of an amputation. I have to-day been assured by Dr. M'Dougall, that the last time he saw him, now some months ago, he walked almost without defect; and he adds, "The operation in his case I considered a great success."

Since the successful result attained in this case, I have operated in an exactly similar manner in five other cases. In all of them, the disease was in a more advanced stage, abscesses having formed, and sinuses existing. One of these cases I now present before the Society, that they may judge of the results for themselves. In this case the operation was performed upon the 23rd of September of last year. The wound was, practically speaking, healed in November, and for the last two months the patient has been walking about.

Upon the last case, I only operated upon the 13th of the present month. The patient is out of bed, and doing well.

In only one of these six cases upon which I have performed this operation, have I had occasion to resort to further interference. In this case, the patient had long been the subject of disease of the tarsus, and, through vain endeavours to avoid operative interference, had become nearly exhausted. The operation afforded complete relief; but as the system of dressing the whole cavity of the wound was abandoned from the first, out of regard to the patient's sensitiveness and weakness, the lines of incision closed, and matter began to bag in the deeper parts of the wound, occasioning pain, and exciting anxieties in the patient's friends lest disease was about to return. I was induced, under these circumstances, to resort to amputation at the ankle-joint, and was thereafter chagrined to find that there was no condition which should not have admitted of sound cicatrization, had the method of dressing the cavity of the wound been rigidly carried out.—*Edinburgh Medical Journal*, May 1874, p. 963.

44.—ON BLOODLESS SURGERY.*

By Dr. FR. ESMARCH, Professor of Surgery in the University of Kiel.

During my present visit to England and Scotland, I have often found occasion to speak with other surgeons on bloodless surgery. I found that some were but imperfectly acquainted with the method; that others applied it, but not

Address delivered at the Clinical Society on October 9th.

in the right manner; that others again attached no importance to the avoidance of hemorrhage during an operation.

This experience induces me to address a few words on the subject to this Society, because I hope to find many present who have acquired similar views to my own on the value of the method, and because I am convinced that it is scarcely possible to speak too often or loudly enough on the influence which this method must exercise on what we call good fortune in surgery. Without doubt, the best proof of the value of any method is afforded by the influence on the mortality after the greater operations, and especially after amputations of the limbs.

I lately compared the results obtained in my practice after operations performed bloodlessly with the recently published results of operations performed by other surgeons, and I found that my results were much better than the best of these, including even those in which the antiseptic method had been strictly followed. But you may very properly object to this, that it is not conclusive to compare the statistics of other hospitals or surgeons with my own, because the circumstances in different hospitals, and the cases as well as the treatment are so different, that they do not admit of comparison without a detailed account of the individual cases.

Much more important, undoubtedly, must be the comparison between cases occurring in my own practice and performed in the same hospital previously to the application of the bloodless method and afterwards. This comparison I am able to offer.

I have put together the statistics of the operations I performed during the last six years, and have found the most striking results. I shall only mention to you, as an example, the statistics of the amputations of the thigh and the leg. Of 88 amputations of the thigh performed in the first five years, there died 37, or 42 per cent. Of 67 amputations of the leg, there died 19, or 28.3 per cent. After the adoption of the bloodless method, there died, of 13 amputations of the thigh, only 1; and of 12 amputations of the leg only 1; so that the proportion of fatal cases in amputations of the thigh and leg together is brought down from 36 to 8 per cent.

Even admitting the error which may result from the difference of the numbers compared, I am of opinion that these statistics afford such striking evidence of the value of bloodless surgery, that no one should neglect this method in cases to which it is at all applicable. Nor should its use be limited to operations on the extremities, but it should be extended to other regions with such special modifications as each case may require. For instance, I at one time believed it to be impossible to use it in amputations at, and in excisions of, the shoulder-

joint; but a few trials showed nothing to be easier. For three operations, it suffices to pass the elastic tubing under the armpit, and to have it tightly held over the shoulder by the hand of an assistant, replacing the latter by a clamp in more protracted operations. In some cases, however, compression by aid of the elastic tubing does not suffice to prevent the afflux of arterial blood, and in these other means for the prevention of hemorrhage must be resorted to. Allow me to detail a case of this description. In the course of last summer, a man between fifty and sixty years of age, with a tumour of the size of an ostrich's egg filling his right armpit, was admitted into my hospital. The tumour had attained this size in the course of two years; and, as it caused great pain, and quite incapacitated the patient from work, its removal at all hazards was wished for. The tumour was firmly wedged in between the chest and shoulder-blade, to the anterior surface of which it seemed to adhere, all movements of the scapula being communicated to the growth, which was unaffected by rotation of the head of the humerus. The presence of severe pain in the arm led to the supposition that adhesions existed between the growth and the nerves in the armpit. Microscopic examination of a piece of the tumour removed by means of an exploring trocar showed it to be a myxosarcoma. Growths of this nature are known frequently to take their origin in nerve-sheaths; and, as risk of rapid relapse in such cases can only be obviated by the operation for their removal including the surrounding tissues, even if these be apparently healthy, I considered that indications existed for removal of the whole arm with the scapula. The elastic tubing could not be applied in this case, nor could other means of compression be resorted to, as the pulsation of the subclavian artery was not to be felt, the tumour having pressed the shoulder upwards, and thereby considerably deepened the supraclavicular fossa. I therefore determined to tie the subclavian artery. I commenced by bandaging the arm up to the shoulder with elastic webbing, and then removed the outer two-thirds of the clavicle, for the purpose of exposing the artery, a course rendered necessary by the altered relation of the parts. Both subclavian artery and vein were then ligatured and divided. The cords of the brachial plexus, which were tensely stretched, and which partly entered into the substance of the tumour, were then cut through. I then made anterior and posterior skin-flaps, and quickly removed scapula and arm with hardly any loss of blood, only a few vessels in the divided muscles requiring ligature. The wound was united by sutures and dressed with carbolic oil. The reaction was moderate, and healing was nearly completed at the time of my leaving Kiel.

The applicability of the bloodless method is, however, not limited to the extremities. Tumours situated in other parts of the body, provided they be superficial, can also be removed without hemorrhage. For instance, in operating upon erectile tumours of the scalp in children, all bleeding can be prevented by compressing the surrounding parts by means of steel rings mounted on handles. In operations on the trunk, the same object can be attained by a thick ring of India-rubber secured by elastic tubing. Operations on the male genitals can also be performed bloodlessly by the employment of a slender piece of elastic tubing, made to encircle the root of the penis and scrotum. It was thus possible for me, for instance, to perform the following operation, the execution of which I should not have attempted under former circumstances.

Last winter, an old man presented himself for admission, suffering from epithelial cancer of the penis of several years' standing. The anterior surface of the scrotum, and the penis within an inch of its root, presented a mass of cauliflower excrescences about the size of the palm of the hand, the seat of offensive discharge, and the occasional source of copious hemorrhage. There was a narrow fissure in the centre of the growth, through which urine was passed with difficulty. The inguinal glands on both sides were enlarged and adherent. The patient was so weak and anæmic that I could not have ventured on operative interference in this case without the bloodless method. With its aid, however, I was enabled fearlessly to undertake an operation, the immediate effects of which were most satisfactory. I began by passing a piece of elastic tubing of the thickness of a little finger round the root of the penis and scrotum; I then crossed it over the symphysis pubis, then passed it backwards, crossed it again over the os sacrum, and finally secured the ends over the abdomen. I then removed the whole growth, including the penis and anterior wall of the scrotum, without losing more blood than was contained in the parts prior to constriction; the vessels being easily recognised and carefully secured after division. I then removed, with all possible speed, the enlarged glands, together with the integument, from both inguinal regions; separated the crura penis, which were found to be endurated, from their attachment to the os pubis, dissecting them carefully from the posterior and healthy part of the cavernous body of the urethra; and then divided the posterior surface of the scrotum by a median incision, at the posterior angle of which I fixed the remaining part of the urethra by sutures, and finally covered the wounded surface anteriorly and in both inguinal regions with the scrotal flaps. The old man had lost but little blood, and was very well after the operation. The large wound healed without any unfavourable

symptoms, and he was about to be discharged to his home, when suddenly fever and symptoms of pleuritic effusion supervened. The patient died in a few weeks of exhaustion. The necropsy showed extensive cancerous deposits in the lungs and pleuræ, with effusion into both pleural cavities.

Gentlemen, I am afraid I have trespassed too much on your time and patience by detailing these cases. I will, therefore, only add in conclusion that, in the three hundred cases in which I have used the bloodless method, I have met with no evil consequences which could be attributed to it. It may, perhaps, be of some interest to state that the longest operation which I performed by this method lasted two hours and a quarter. It was a case of necrosis of both tibiæ, with suppuration of the knee-joint on one side, in which I first removed many pieces of dead bone from one tibia, and then performed resection of the knee-joint; my assistant being at the same time engaged in operating for necrosis on the other limb. This is a different case from that already reported in one of my papers on the subject.—*British Medical Journal*, Oct. 17, 1874, p. 491.

45.—ON BLOODLESS OPERATIONS.

By WILLIAM MAC CORMAC, Esq., Surgeon to St. Thomas's Hospital.

To those who have not actually seen in use the simple mechanism invented by Professor Esmarch for the prevention of hemorrhage during operations on the extremities the title of this paper may perhaps seem somewhat exaggerated. It is nevertheless true that it is now possible safely and securely to perform almost any operation on the limbs without the loss of a single drop of blood.

It was not until August in the past year that the brief record of Professor Esmarch's procedure, as related to the second congress of German surgeons held in Berlin last Easter, became known to me. The plan there detailed struck me as being at once so simple and so likely to prove effectual, that I hastened to put it into practice at the earliest opportunity.

It happened that I had under my care at the moment a little delicate girl of six years, suffering from necrosis of the shaft of the tibia. On my way down to the hospital on August 16th, 1873, I purchased about three feet of india-rubber rope, half an inch in diameter, and an elastic bandage two inches and a half wide, and six yards long. To the end of the india-rubber rope I fastened a couple of hooks, and the apparatus was complete. Whilst the little patient was being chloroformed I bandaged the limb from the toes to the mid-thigh, keeping the elastic bandage on the stretch whilst it was being applied, so

that the entire limb was firmly and equably constricted. So soon as the anæsthesia was complete the india-rubber rope was applied close to the upper border of bandage, being turned around the limb three or four times, and the ends hooked together, the metal hooks resting on the subjacent rubber, and not pressing upon the soft parts. If the rope be long relatively to the circumference of the limb it is always possible to graduate the constricting force sufficiently, and it is important to know that the amount needful completely to shut off the current of blood is not so considerable as one might *à priori* suppose. Esmarch uses a tube three quarters of an inch in diameter. When it is desirable to distribute the pressure over a larger surface the tube is preferable to the solid rope. If economy of space, be an object, as in an amputation high up in the thigh, then the smaller sized rope possesses the advantage. In operations upon the upper extremity, the male genital organs, or the fingers, a smaller-sized tube or rope should be selected. Before applying the rope an interval of a minute should be allowed for the compression to empty the limb completely of blood. The elastic bandage may now be unrolled. By these simple means it will be found that complete local anæmia is produced. The bandaging has driven, practically, all the blood out of the limb, whilst the circular compression above completely prevents any from gaining admission during the time it remains applied.

The limb thus treated appears of waxy pallor, and the temperature becomes greatly reduced.

The subsequent steps of the operation were now performed in the case before mentioned with the greatest ease, and a moderate-sized sequestrum removed from within a casing of newly deposited bone. The wound throughout remained absolutely unstained with blood. Neither during the section of the superficial nor of the deeper structures did a drop of blood appear.

The wound having been firmly plugged with lint, and a bandage applied, the india-rubber rope was removed and a perfectly bloodless as well as painless operation completed, the first of its kind performed in England.

It appears to me difficult to overestimate the value of so simple and certain a method of avoiding hemorrhage during operations on the limbs, but it may be of interest to criticise its scope and the precautions necessary in individual cases.

But first one may say a word on the ever-disputed question of priority. The best answer perhaps is that, whilst previous somewhat similar methods have not to any considerable extent been adopted, or taken root at all, this plan of Professor Esmarch has spread so rapidly and widely that now, after the

short interval since it was made first known to English surgeons, there are but few hospitals throughout the country in which it has not been tried and approved of. In France too where foreign surgery is not always welcome this procedure is being widely adopted. In the principle there is nothing new. Attempts more or less successful have been constantly made by surgeons to spare the flow of blood during operations, and even to save the blood otherwise lost in an amputated limb. The last have generally consisted in the elevation or bandaging of the limb about to be amputated prior to the application of the tourniquet or other compression. But these methods are not complete, and one form of tourniquet after another has been rejected. Indeed by many digital compression has been uniformly adopted in its stead. How often I have felt anxiety about the steadiness of my assistant's fingers compressing a main trunk during a great operation it is unnecessary to say, or how disconcerting to all concerned, and dangerous to the patient, a sudden gush if what Dieffenbach called "dämonische Blut" is under circumstances such as these. The great and incomparable advantages of Esmarch's plan are its completeness and its simplicity.

Stromeyer went very near it when in 1853 he tightly bandaged an arm, the subject of traumatic brachial aneurism, and applied a tourniquet above, before tying the brachial artery at the seat of injury. Langenbeck mentions in the "*Berliner Klinische Wochenschrift*," December 29th, 1873, that for years past he has been in the habit, in cases of weakly patients requiring amputation of the thigh, of applying a wet bandage tightly to the limb from the tips of the toes to the place where the tourniquet is to be applied.

^t Mr. Clover made a similar proposal on which Mr. Erichsen acted some years ago. In France and in Edinburgh, and perhaps elsewhere, the limb has been simply raised to empty it partially of blood before applying the tourniquet. A very long time since, in 1806, namely, Sartorius, before proceeding to the extensive operation he deemed necessary in order to divide the tendo Achillis, mentions that "the limb was first enveloped with a roller bandage and a graduated compress and tourniquet applied to the femoral artery." He states, "but little hemorrhage took place." Finally, Professor Vanzetti, of Padua, claims on the part of Dr. Silvestri, of Vicenza, the priority of application of the elastic ligature and elastic compression in surgery.

It appears that Silvestri actually used bandaging and the elastic rope in amputations as early as 1862. But no general cognizance of his experiments was taken, and it will be noticed that in most instances they were only had recourse to in ampu-

tations of limbs. Professor Esmarch's simple means is applicable to all kinds of operations. In none perhaps is it more strikingly useful than in operations for necrosis of bone. In these the bleeding is sometimes excessive and difficult to restrain, whilst the presence of blood in the generally deep wound obscures the surgeon's view, and necessitates the constant sponging out of the cavity. The duration of the operation is thus necessarily much prolonged and its difficulty enhanced. In the interesting paper published by Esmarch in the "*Sammlung Klinischer Vorträge*," he mentions having performed a necrotomy on both tibiæ simultaneously, with aid of his assistant, Dr. Petersen.

After operations such as these the wound may be plugged before removing the constricting rope, and no bleeding takes place; but after amputation or excision it is impossible thus to secure all the bleeding points, and after securing the principal vessels the elastic rope must be removed before closing the wound.

Then a curious phenomenon is observed. A rapidly travelling blush extends down the limb, which in a few seconds presents an erythematous redness, and smart capillary bleeding occurs from the wound, whilst the patient is roused from the semi-narcotism he is in, and apparently suffers much pain.

Probably the previous constriction has induced some amount of vaso-motor paralysis, thus accounting for the redness of the surface. Although the capillary oozing is free, it is never difficult to restrain by suitable applications, while all visible bleeding points should be tied or twisted.

Another curious condition which may be observed in operations on the bones is their apparent bloodlessness, as well as that of the soft parts. In an excision of the knee performed by Mr. Arnott, and an excision of the elbow performed by myself, the bones seemed in a sense dry, and pale. The elastic bandage cannot, of course, force the blood out of the Haversian canals, but it is probably sucked out after its removal, into the emptied vessels of the soft parts, when their natural resiliency is allowed to act, and the blood remaining in the bone is distributed evenly over the entire limb. It is not a mere stasis of blood in the bone, there is certainly anæmia of it likewise. Great facility is afforded to the operator in consequence of this absence of bleeding from the bone, by enabling him accurately to define the limits of the diseased structures. Allowance must, of course, be made for the somewhat altered appearance of the exsanguine parts.

I have not observed any tendency to secondary hemorrhage in the cases I have operated upon, nor any nervous suffering by reason of the forcible constriction of the limb, nor has Esmarch,

in the now large number of cases in which he has employed his method. The subsequent healing of the wound is certainly not interfered with. Esmarch believes that rapid healing is even promoted.

In the paper before cited he mentions having used the method in eighty-seven cases, twenty-one of which were amputations or excisions, namely: six amputations of the thigh, eight of the leg, one disarticulation of the shoulder, eight resections, thirteen necrotomies, and five removals of tumours, the rest being minor operations.

Of these eighty-seven patients only four died, and in most of the amputations the stumps healed by primary union, with scarcely any traumatic fever.

Von Langenbeck has pointed out that care must be taken in applying the india-rubber rope to the upper arm, as it may unduly press upon some of the rather superficially placed brachial nerves. In two cases he observed temporary paralysis of parts supplied by branches of the median nerve, which induced him to substitute in the upper extremity a few turns of elastic bandage around the limb in place of the rope. With this simple modification he has performed excision of the wrist joint and several other operations quite satisfactorily, and without any subsequent lesion of the nerves.

In cases where there are diffuse abscesses in the limb, or any putrid or gangrenous deposits, the use of the elastic bandage would certainly not be free from risk of pressing the septic materia into the general circulation. The same may be said of soft textured tumours, or cancers. In these cases it will be sufficient simply to elevate the limb before applying the elastic rope, and thus will all danger be avoided.

It has been urged occasionally that some loss of blood is desirable in certain cases. Such cases are, in my opinion, very rare indeed, and it cannot be difficult, when they occur, for the surgeon who desires to do so to allow any given quantity of blood to escape that he may wish the patient to lose. One great advantage to be gained by this method is the almost complete exclusion of sponges and sponging from the operation wound. One can never be sure, however carefully they be disinfected, that no infective material is retained in them. Besides, the mechanical obstruction caused by the constant sponging out of bleeding wounds is removed, and the operator can proceed unretarded. Esmarch does not use sponges at all, but irrigates the wound with a one per cent. solution of hydrochloric acid, which he prefers to carbolic acid as a disinfectant.

Possibly this method of Esmarch may prove useful in the treatment of certain aneurisms. We know now how readily

aneurisms may often be cured in a few hours by the complete arrest of the flow of blood through the sac. For instance, in a popliteal and femoral aneurism we might easily and perfectly arrest the current of blood, and if the limb were previously, to some extent at least, if not completely, emptied of blood, the aneurismal sac would be allowed the opportunity of contracting in size, which would certainly prove a powerful adjunct to the compression in furthering a cure.

That the elastic rope might be applied sufficiently long to ensure the desired result would seem certain from the experiments of Cohnheim, who has proved that the circulation may be completely interrupted in warm-blooded animals for as long a period as six or eight hours, without any permanent bad effect ensuing. So far as it has been tried on the human subject, no accident to the circulation, such as thrombosis or inflammation of veins, or any permanent injury to the innervation of the part, has yet occurred as the consequence of even protracted elastic compression of a limb. In the treatment of wounded blood-vessels, and of traumatic aneurism, when it is desired to ligature the vessel at the seat of injury, Esmarch's plan would afford most certainly great facilities. Or, better still, the artery might be securely twisted, and the same thing might likewise be done in the continuity of arteries for aneurism. The vessel, after being exposed, might be cut through, twisted and each end allowed to retract. In an operation for aneurism, where the sac requires to be opened and the ends of the vessel entering and leaving it sought for and secured, Esmarch's method would prove invaluable.

Dr. Leisrink ("Deutsche Zeitschrift für Chirurgie," B. IV.) ligatured the anterior tibial artery in the upper third of the leg on account of traumatic aneurism, a feat scarcely impracticable without the aid of Esmarch's plan.

How difficult such a small operation as finding a piece of needle or splintered glass broken off in the palm of the hand or sole of the foot may often prove, surgeons have frequent experience. The part being first rendered bloodless, however, a successful search becomes easy. The discovery of lodged bullets or other foreign bodies, the methodical and minute examination of diseased or injured parts, to ascertain if amputation or some less serious operation be needed, is also rendered less difficult and uncertain.

Further experience will, doubtless, be required before the full scope of this method, its advantages and disadvantages, can be definitively settled; but amply sufficient has already been recorded to show that we are indebted to Professor Esmarch for a complete and simple means of facilitating the performance of a large class of surgical operations, and of

rendering them both safer to the patient and easier of execution to the surgeon. We certainly owe him thanks for this.—*St. Thomas's Hospital Reports*, 1873, p. 43.

46.—TRUE ANCHYLOSIS OF THE HIP-JOINT.

By LAMBERT H. ORMSBY, Esq., Surgeon to the Meath Hospital, Dublin.

[Complete ankylosis of the hip-joint is by no means so rare as is commonly supposed. There are seven well-marked specimens of this affection in the museum of the Royal College of Surgeons of Ireland and many in the various London museums.]

True bony ankylosis can occur in the scrofulous subject, and attention and care may occasionally overcome the disposition to excessive flexion and adduction of the limb by keeping it in the straight position during consolidation. It may also arise in those unfortunate people who are bed-ridden for years, or in those suffering from some peculiar form of gouty or rheumatic diathesis. There is a skeleton of a man who had ankylosis of nearly every joint in his body in the Museum of the Royal College of Surgeons in Ireland, a full account and previous history of which appears in the Museum Catalogue; this, however, was of the fibrous variety in many of his joints. The diagnosis between true and false is sometimes not so easy to determine in the fibrous; it may be very firm, and closely simulates the true, but placing the patient fully under the influence of chloroform seems to me to be the best method of clearing up the difficulty, and then, if bony, very often that firm sensation will be given to your hand, and in moving the limb you move the pelvis with it; you may also do this in the false variety, but not to the same extent; the limb may be ankylosed straight, but more frequently the thigh is bent up and flexed on the abdomen, causing great deformity and perfect uselessness of the limb. This may be also followed by permanent flexion and deformity at the knee and ankle, and when all inflammation, abscess, &c., and constitutional disturbances about the joint have subsided. It is a matter of great moment to consider what is the best and safest operative treatment to be adopted for such cases.

We are not confined, I may mention, to any one particular mode of operating, but any of the operations for bony ankylosis before mentioned might be performed at this joint with certain modifications to suit the existing circumstances of the case; but I will mention three procedures which have been recommended and performed from time to time, all said to be peculiarly suited for bony consolidation in this articulation.

1.—*Dr. Rhea Barton's Operation.*—Dr. Barton, of Philadelphia, proposed to *establish a false joint after section of the bone*; he first performed the operation in 1826, on a sailor, named John Coyle, æt. 21, who had fallen down the hold of a vessel, and in doing so fractured his thigh bone. Angular union took place, with great deformity, and the limb being perfectly useless, the man prevented from working, as the thigh was flexed, and leg carried over opposite side. Twenty months after the receipt of the injury, the operation was performed, and so far as it went, was attended with the greatest success.

Description of the Operation.—A crucial incision seven inches in length, and five inches in horizontal direction, made over the great trochanter; a fine saw was then inserted, and the bone was completely sawn through, just between the two trochanters. The natural position of the limb was at once and easily restored in the case operated on by Dr. Barton, the man was able at the expiration of two months to put his foot to the ground, and after four months he could walk a considerable distance; this man then gave himself up to debauchery and dissipation, &c., and he died of consumption thus produced nine years after the operation. He got an attack of inflammation two years before his death in this new joint, which was therefore ankylosed. This operation has frequently been repeated by surgeons with great success; by this procedure the bone must be laid bare, and motion must be constantly set up after division to prevent reunion taking place. A kind of capsular ligament is formed, and a rounded head is formed on the lower portion of bone, and an excavated cavity on the upper or fixed portion of bone, and a false joint thereby produced.

2.—*Description of Sayre's Operation.*—Dr. Lewis Sayre, of New York, performed his operation as follows: For the same object, an incision being made six inches in length, over the greater trochanter in the axis of the limb, a transverse section of the femur of elliptical form removed just above the lesser trochanter by the means of a chain-saw. Dr. Sayre performed his first operation after this method on a man æt. 26, on the 11th June, 1862, and in six months afterwards he is reported "to stand on either leg without crutch or cane," and continued to use his limb for many years afterwards with practical agility.

Dr. Sayre's second operation did not turn out so well, at least the patient did not; it was performed on a girl æt. 24, on the 6th of November, 1862, and she died on the 17th May, 1863, from an attack of pleuro-pneumonia; the wound, however, and discharge ceased four months after the operation. On post-mortem examination tubercular deposits were found

in each lung, and a large abscess in left lung. The artificial joint was found to be provided with a complete capsular ligament, and the divided surfaces of bone were found tipped with cartilage, and furnished with a synovial membrane.

3.—*Mr. William Adams' Operation.*—Mr. William Adams, of London, has suggested and performed successfully, a most important modification of these operations, and his differs, in my opinion, particularly in three respects—viz.: 1st. The way it is performed; 2nd. The situation in which the bone is divided; 3rd. The instruments used in the operation. It is performed subcutaneously, situation being the neck of the thigh-bone inside the capsular ligament, and all that is required is a long-bladed tenotome and a narrow-bladed saw with a good handle; the saw is three-eighths of an inch in width, and with a cutting edge an inch and a-half in length. The following is Mr. Adams' description of the operation. He first performed on a man called Luke Bristowe, æt. 24, in the Great Northern Hospital, on the 1st of December, 1869, and divided the neck of the thigh-bone, inside the capsule, for ankylosis:—

“I entered the tenotomy-knife a little above the top of the greater trochanter, and carrying it straight down to the neck of the thigh-bone, divided the muscles and opened the capsular ligament freely; withdrawing the knife, I carried the small saw along the track made, preserving this by pressure of the fingers straight down to the bone, and sawed through it from before backwards; the section of the bone was accomplished in five minutes. No hemorrhage followed, and I immediately applied a compress of dry lint retained in position by strips of plaster and a bandage; as soon as the bone was cut through the leg moved freely in all directions, but before it could be brought into straight position it was necessary to divide the tendons of the long head of the rectus and of the adductor longus muscles, and to cut through the tensor vaginae femoris muscle. The limb was fixed in a straight position and bandaged to a long interrupted Liston's splint; no inflammation whatever followed the operation; no swelling or redness of the skin or any deep suppuration, but the wound healed slowly.” It is not necessary here to follow up the history of the case, suffice it to say that he recovered with a comparatively straight limb, which enabled him to walk with great ease, but the bones united and became ankylosed, a danger that is always likely to occur, but whether or not, you change a limb by the operation from being most useless and deformed to one that is straight, at all events, and useful for locomotion. There is generally a good deal of difficulty experienced in sawing the bone, but it generally can be overcome and effected with a moderate amount of gentleness and patience. For more in-

formation I refer you to Mr. Adams' before-mentioned pamphlet. Mr. Adams' operation has been since repeated by various surgeons, among whom may be mentioned, twice by Mr. Jessop, of Leeds; once by Mr. Jordan, of Birmingham; once by Mr. Somers, of Brighton; once by Mr. John Croft, St. Thomas's Hospital; once by Mr. Hardie, of Manchester, all of which were successful, with the exception of one case that was performed on a child. As soon as practicable after such operations and the subcutaneous incision healed, passive motion should be set up and every day for a few minutes applied; weights also are useful, attached to the feet to prevent union taking place, which is as well to prevent if possible, and if much pain is produced on moving the limb, with a view to set up passive motion, chloroform should be administered.—*Medical Press and Circular*, May 20, 1874, p. 417.

47.—ON TREPHINING IN DEPRESSED FRACTURE OF THE SKULL.

By WILLIAM S. SAVORY, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

[Almost all surgeons would be in accord on the question of interference in the majority of cases of depressed fracture of the skull, whilst differences of opinion would exist about the treatment of other cases. There are certain rules, so far as they can be laid down, by which the judgment of surgeons is usually guided.]

The question of operation is almost always associated with fracture of the vault of the skull, not only because depressed fractures occur much more often in the vault than about the base, but also because the operation itself is only applicable to one or two portions of the base. Fractures of the skull may be classified in the following manner, which appears to me to possess especial advantages with reference to the question of operation:—1. Fractures with or without depression of bone. 2. Fractures with or without symptoms of compression. 3. Simple fractures and compound fractures. This classification admits of great variation, and you will see how it is possible and also useful to ring the changes upon these three divisions when we come to the question of when the trephine should be applied.

There are two chief forms of fracture with which we have now to do: the ordinary split or fissured fracture, which is often starred, usually comminuted, and which affects us at present only as it is at the same time depressed; and the punctured fracture, the most important feature of which is

that, whilst the outer table of the skull often presents but a slight depression, the inner table is much more severely broken and more deeply depressed. Forms of some of these fractures are well exhibited in these specimens from the shelves of our museum. Here is a skull, in the parietal bone of which there is a perfectly well-defined circular depression affecting both the inner and the outer tables, shelving smoothly down in the latter from all sides towards the deepest part of the dent—a thoroughly good example of the uniform, shallow, or spoon-shaped depression. Here is another, a depressed fracture of the occipital bone of a child of some twelve or fourteen years. It also is circular in form, gradually dipping down; but in addition to this, although not a true punctured fracture, it shows the main feature of such fractures—its inner table is much broken, and the outer scarcely more than bent inwards. This specimen, again, shows a much larger and more irregular piece of the parietal bone driven inwards, but not deeply. But look at this portion of the parietal bone of a young child. See how a small hole is, as it were, punched out with a trephine, so that at first sight it might be thought to be a specimen showing the effects of the operation. Look at it more closely, however, and you will then see that it is oval rather than round in shape; that on one side there is the slight fissure of a fracture extending from its border; that on the inside of the bone there are, attached to the margins of the aperture, and almost completely surrounding it, larger and smaller fragments of bone pointing directly inward toward the brain and its membranes. A better example of depressed fracture with the fragments driven in endways or edgeways it is impossible to find. Its history, too, is in some ways as typical as the specimen itself. A chimney-pot fell from the roof of a house upon the child's head. In spite of the heaviness of the blow and the hole bored completely through the skull, it is said that there were scarcely any symptoms during three weeks after the injury. Then inflammation of the brain took place, and speedy death was the result. It occasionally happens that the outer table of the skull alone is depressed, the inner escaping uninjured, or almost uninjured. Such may be the case when a fracture is sustained over the frontal sinuses, or over the mastoid portion of the temporal bone. Cases of this kind, of the former injury especially, are not uncommon in the wards.

The consequences of such fractures as have just been described fall naturally into a two-fold division: immediate and remote (or indirect). The direct consequence of the compression due to a depressed fracture is to produce coma, often mingled in various degrees with concussion, but marked especially, as you well know, by insensibility more or less profound, sluggish cir-

ulation, laboured respiration, and, as the case advances, increasing paralysis of muscle. Such a condition may quickly pass into death, by way of apnoea, or the patient may regain consciousness more or less completely, and in time become subject to the remote consequences of the injury. Fully to describe these would require more time than we can devote to this lecture. Shortly, they are all those produced by irritation or inflammation of the contents of the skull, giving rise to pain and fever, muscular twitchings and contraction, convulsions, delirium, coma, paralysis, and death. The consequences depend in part upon the nature of the fracture: thus, fractures of considerable extent and deep uniform depression are likely to be followed by coma; fractures which are much comminuted, where the fragments are driven in edgeways and the inner table especially damaged, are more likely to give rise to irritation or to set up inflammation—scarcely less likely, however, in the first instance to produce coma.

Now, let us consider in what fractures and under what circumstances we may be called upon to trephine or to decide against the operation. We shall here find the advantage of the rough classification laid down in the beginning of the lecture. I may state that under the term “trephining” all operations for the elevation or removal of depressed bone are included. In fractures without depression and without symptoms, whether simple or compound, the answer is obvious: Do not operate. In a simple fracture with depression, but without any symptoms of compression, the answer is also No, unless it happen that the fragments have been driven in edgeways or edgeways, when it is generally expedient to operate, in order to obviate or render less likely the remote effects we have seen are so liable to follow this kind of fracture. If there be a wound communicating with the fracture, the rule of treatment remains the same, the operation being rendered more obvious and easy than in the last case. Supposing that symptoms are present, but that there is no external wound, and that no fracture or depression can be detected, the treatment becomes more difficult. The symptoms may be due to other causes—to internal hemorrhage, to laceration of the brain, to a fracture in some part remote from the seat of injury. We, therefore, wait for a while and watch the patient carefully, ready at any time, if the symptoms do not give way, and if no contra-indication appear, to cut down and explore, our further treatment being guided by what we then find. The same case may occur, but with a wound of the scalp. Even under these circumstances, the rule is not to interfere at first, but to wait as before, keeping strict watch and noting whether the symptoms diminish in severity or whether they continue or increase.

As complete an examination as the wound will permit is of course always made. The next class of cases is those in which both depression and symptoms of compression are met with. Here, perhaps, the rule is, even when the fracture is simple, to cut down and raise the depressed portion. Where such a fracture is compound we rarely hesitate to operate.

Much importance is laid upon the question of whether the fracture be simple or compound in standard works on the subject. I think too much stress has been put upon this point. That it should exercise a certain influence is, of course, perfectly right, for it is not a light matter under any circumstances to convert a simple into a compound fracture, least so when the skull is concerned. But when, on the other hand, the advantage which may be gained by a successful operation is considered, and the imminent peril in which the patient is placed in certain cases if no operation be undertaken, I think there can be no doubt that the right course to pursue is to cut down upon the bone, and explore. The greatest difference of opinion exists perhaps as to the right treatment to be pursued when there are symptoms of compression, as coma, but without clear evidence of depression. I agree in the main with what Mr. Erichsen has said on this subject—that when such symptoms exist, in all cases of doubt, an incision should be made down to the bone, and the state of the skull examined. Further proceedings will depend almost entirely upon the condition of the skull when thus laid bare. There is one form of fracture in which the rule is constant to trephine at once. This is the punctured fracture, in which the inner table is usually much comminuted. It is of no consequence whether or not there be immediate symptoms of compression; the operation is not performed on account of these, but in order to prevent, if possible, the irritation and inflammation almost sure to be set up by the small and often sharp splinters which are formed.

Such are the ordinary rules by which we are guided in deciding whether to operate in depressed fracture or not. They are liable to be altered by certain circumstances, of course, and the circumstance of age is one which exercises as great an influence as any. The cases of children are decidedly exceptional. Strange as it may seem, although the brain is extremely sensitive to disease in childhood, and is easily disturbed by injury, the part disturbed or encroached on recovers more rapidly than in the adult. The “power of accommodation” is greater in the child. The injured part is able to adapt itself to the altered conditions far more quickly than at a later period of life. This is a subject well worthy of attention—a subject up to the present time very little worked at.

I will now relate the case which has formed the basis of this

lecture, and you will see that, in accordance with what has just been said, the ordinary rule of treatment was deviated from, and the operation, which would have been practised upon an adult, was deemed unnecessary in a child.

On the 2nd of March there was admitted into Lucas ward a child eight months old. The mother, who brought her, stated that she was carrying the child down stairs, and that when some two or three steps from the bottom she had slipped and fallen. The head of the infant was struck in the fall, she believed, by the corner of the last bar of the banisters. Finding there was a deep dent in the head, she at once brought her to the hospital, although she did not think there could be any danger from the injury, as the child scarcely seemed to suffer at all, and even took the breast immediately. On admission, however, the child was quite unconscious, the face was pallid, the pulse small and scarcely perceptible, the skin cool and very moist. In the posterior portion of the left parietal bone, just above the squamo-parietal suture, was a deep depression in the skull, wide enough to lay the ends of two fingers in, and about a finger's breadth in depth. At the bottom of this could be felt the rough edge of the fractured bone. There was no wound of the scalp. I was sent for, but before I had time to arrive the symptoms of compression had passed off, and the patient was sitting up in its mother's arms. I therefore decided not to interfere, but to watch the case carefully. The next morning the child sat up, appeared to take notice of things as usual, cried, laughed, sucked, and seemed so well that about eleven o'clock the mother insisted upon taking it home. About half-past two she was again admitted, having hastened down from the City-road as quickly as possible, much frightened, because about half an hour previously the child had turned up its eyes, and worked its hands and mouth violently for some seconds, after which it had fallen into a deep sleep. It was again in a similar condition of coma to that noticed on its first admission. I held consultation with my colleagues, whose advice concurred with my own opinion, that no operation should for the present be performed. Soon after we had seen it it recovered consciousness, and from this time until the afternoon of the 5th presented no signs of cerebral mischief. It then had a convulsion, affecting both sides of the body, lasting only a few seconds, followed by coma of two or three hours' duration. On the afternoon of the 6th a convulsion again occurred, which was followed at intervals of about an hour by similar convulsions through the rest of the day and night, until five o'clock on the morning of the 7th. The fits then passed off; the child appeared entirely to have recovered, and during the afternoon it was again taken away by its mother.

In this case there was therefore a simple fracture with depression, and with symptoms. The first signs of compression had passed off before I saw the child, but they were succeeded within a few hours by the more remote effects of the injury, effects due to irritation of the brain by the rough edges of the depressed bone acting through the dura mater. Bearing in mind, however, the wonderful manner in which young children become accustomed to such a condition of parts, seeing how well the child was between the attacks of irritation, and how quickly it had recovered from the first effects of the injury, we undoubtedly decided rightly in not trephining. It is probable that the child will recover completely, that all symptoms of irritation will, in time, subside, and that even the depression of the skull will become less as she grows older, the bone being gradually raised to a nearer level with the surrounding parts.

But although children often recover well, even from severe depressed fractures, it may be asked with regard to adults, Why not trephine in every instance of fracture where there is a depression? What can be expected from such a case if left to give rise to the secondary effects which have been mentioned? In the first place, the symptoms of depression, although associated with fracture, may be due, not to it, but to some other cause. It may be that the brain has been severely bruised or otherwise injured at some point remote from the seat of the fracture; that blood has been effused in large quantity upon the surface or into the substance of the brain, and that the symptoms are dependent upon these causes, not upon the pressure of the bone. Again, in adults sometimes, as in children, the symptoms may subside after a shorter or longer time, through the same power of accommodation to the altered pressure. Lastly, the operation itself is by no means free from risk. Even with the greatest care, small chips of bone may be detached, may be left upon the surface of the dura mater, and may produce the same inflammation to avoid which was one of the great objects of the operation. The mere interference with the dura mater during the removal or raising of the bone may be enough to light up inflammation. Another result which occasionally, though rarely, follows trephining is well shown in these specimens. They both represent what is called *hernia cerebri*—the protrusion of a mass of brain-substance, mingled with products of inflammation, through the aperture in the skull. In one case the protruding mass was cut off several times, but grew again; in each case death was the final result; in neither case was the dura mater wounded during the operation, but ulceration of the membranes had taken place.

Nothing, perhaps, is of greater consequence in the question

of trephining than the time which has elapsed since the injury. In almost all cases sufficient time should be allowed for the patient to recover from the mere effects of the blow; it is the persistence of symptoms after this period by which we are guided. At a much later date it may be still right to trephine, even when inflammation appears to have taken place; as, for instance, in some cases of punctured fracture.

As a rule it has happened that men who have had the largest experience in cases of injury to the head have been the least inclined to trephine, although in some parts of England, as, for instance in Cornwall, where mine accidents afford considerable practice in these injuries, and in some of the large towns of the north, it has been the custom to operate very much more frequently than we should be inclined to do. Those who operate thus frequently are most in favour of the operation, believing that it has been the means of saving very many lives. From our own experience we should say that probably many of these would still have recovered if they had not been trephined. However this may be, such experience as theirs affords important evidence on the risks of the operation itself apart from any other injury.—*Lancet*, Aug. 1, 1874, p. 147.

48.—ANTISEPTIC SURGERY.

By the EDITOR OF THE BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

The present condition of the great antiseptic controversy seems to be this. The supporters of the germ-origin of surgical and medical diseases are in a decided minority. This arises mainly from a distrust of purely theoretical considerations as influencing practical treatment, until the theory upon which the latter is founded has been supported by such an array of experiment and fact, and such an accumulation of probabilities, that the induction amounts to a certainty. The germ-theory includes so much more than those diseases of the pyæmic class which are called surgical, and must seek for its proof in the study of physiology, pathology and medicine, as well as in the more elementary natural sciences which embrace an area so much wider than that with which surgeons are practically conversant, that the latter must perforce wait more or less upon the further development of the inquiries which are now actually in progress in these cognate, though increasingly divergent pursuits. The time for deciding upon the proximate cause or causes of pyæmia as of cancer was shown by the extreme divergence of opinion, and even dissonance of facts, among the most eminent speakers in the late pathological

debate on these subjects, *not* to have arrived as yet. "Quot homines, tot sententiæ" may be applied here with force more than usual.

Until that time does arrive it is clear that treatment must necessarily be founded to a great extent upon surmise, and that this will vary with the theoretical proclivities of the practitioner, and to an extent quite as extreme. Among the London authorities at the debate alluded to, the germ-theory was at such a discount that not one could be found to advance it or even to allude to it. Contempt for its pretensions or the fear of the ridicule which weaker brethren might attach to it must have been the cause of reticence so unanimous. A mere uncertainty as to its truth or falsehood could hardly, in an age and a profession so pre-eminently tentative, have effected so curious a manifestation.

But there was no such cold shade over the chemical doctrine of putrefaction or other similar poisonous decomposition of the discharges or of the blood, which was spoken of and argued upon quite as freely as the necessity for the use of some such theoretical expression evidently required. Among the various ways in which those putrefactive changes and tendencies can be counteracted the use of antiseptics of various kinds (and prominent among them, of course, that of carbolic acid) has quite an even chance with any other.

But cleanliness, rest, and drainage, and ventilation and refrigeration, were far more insisted upon than antiseptics in the latest expression of opinion upon the best local treatment of wounds.

Constitutional treatment in cases where operations are imperative and immediate seems hardly to have been thought of, and the previous careful preparation of the patient for operation in such cases as admit of it, although mentioned and insisted on, is not specified in Mr. Callander's paper. Dr. Sansom's practice of the internal administration of some preparation of carbolic acid seems to have been tried by very few, although the safe saturation of the system with the antiseptic, which can be effected by means of the sulpho-carbolates, seems to be undoubted.

At King's College Hospital this method is being put upon trial in pyæmic cases by Mr. Wood, associated with the respiration of carbolised air, and irrigation with carbolised water to the wounded part. The results of experiments upon an issue so difficult to prove require, however, a long period of careful observation by many competent men before they can be thoroughly relied upon. The course of an attack of acute pyæmia is so rapid and violent in its disorganising effects, that it is probable that no remedies have much power in arresting it.

when once established. It would seem wisest therefore to rely upon such measures of prevention and prophylaxis as have proved in cases of typhoid and other fevers more fruitful in success than futile efforts at a so-called cure.

Quinine is a remedy which has been recommended, and may easily be combined with other plans. Of itself it has been tried frequently, even in large doses, and has failed to produce any perceptible effect upon the rigors which accompany pyæmia, and it does not seem to exert any decided antiseptic influence upon the tissues.

An important consideration in the estimate of the value of any line of treatment whatever is its universal applicability under all the varying circumstances of human disease or misfortune. The elaboration and care required by Lister's method is no doubt an obstacle to its ready acceptance, and savours of that peculiar pretentiousness which has been fatal to so many complicated systems of surgical treatment, under the experience that they were seldom necessary even if of some value. To a less extent this feeling applies to Mr. Callender's camel's-hair brush and drainage-tube also. Under the pressure of circumstances frequently exemplified in military hospitals during war, when the conditions against which they are especially directed most prevail, it is evident that they are difficult, if not impossible. Nor can we endow ordinary surgeons and dressers with Lister's or Callender's patient painstaking and prevision. Under many of these circumstances the use of Mr. Hewson's plan of dry earth might furnish its own most powerful justification. Dry clay might be obtainable when carbolic spray producers, prepared muslin, jacquINETTE, cere-cloth, camel's hair-brushes, and winged drainage tubes could not be procured at all or not in sufficient quantity. Alternative methods in the practice of surgery obtain often a greater value under circumstances admitting of their more ready employment, and one of the greatest endowments of the surgical mind lies in that fertility of resource and recollection which can supply an immediate eclecticism in the choice of the remedies most at hand. But this is and doubtless will remain clear that no antiseptic or other application, nor internal remedies, however valuable in themselves as an aid, will supply the want of surgical skill in arranging by the method of operation or dressing for the due drainage of the wound; or the want of foresight in the arrangement of the wards and beds; or the want of cleanliness and care in dressers and nurses; or the want of judgment in the choice and in time of operating, and the manner of feeding and supporting the "*vis medicatrix naturæ*" which is always and must continue to be the prime factor in restoring tissues and repairing injury, and which has furnished and must still

continue to supply the best or sole safeguard of the patient, when man's art, care, and skill are not forthcoming, or fail to do their part towards the sick or wounded sufferer.—*British and Foreign Medico-Chirurgical Review*, July 1874, p. 14.

49.—ON VARIETIES OF PSOAS ABSCESS.

By WALTER RIVINGTON, Esq., Surgeon to the London Hospital.

In cases originating from disease of the spinal column it becomes a question of interest whether the starting-point of the disease is in the vertebræ or in the intervertebral discs. Up to a comparatively recent period I believe that surgical opinion regarded "angular curvature" as almost always the result of scrofulous inflammation and caries of the bodies of the vertebræ, the intervertebral discs being regarded as the innocent victims of the disturbance and destruction arising from the errors and weakness of their osseous neighbours. But more careful pathological inquiry demonstrated the fact that the intervertebral substances were sometimes primarily at fault. There is a specimen in the London Hospital Museum (marked Fb 3) showing destruction of several intervertebral discs with but little implication of the bodies of the vertebræ; and I doubt not that most hospital collections contain illustrations of the same condition. Both Sir B. Brodie and Mr. Stanley have related cases which clearly establish the frequency with which the intervertebral discs are primarily affected; and the records of the sporadic cases which from time to time appear in the journals confirm the view. Mr. Bryant is of opinion that the disease more often commences in the intervertebral discs than in the bodies of the vertebræ. The disease of the fibro-cartilages was regarded by Mr. Stanley as of an inflammatory nature, occasioning softening, splitting of its tissue into threads, and its ultimate disappearance. A single fibro-cartilage may suffer, but the disease "often attacks several of them, and there have been instances of its occurrence through the whole series of fibro-cartilages from the second cervical vertebra to the sacrum." Mr. Stanley met with many instances of psoas abscess, some single and some double, from this cause, and often only a single fibro-cartilage diseased. "In the body of a young man," he says, "from whose thighs two psoas abscesses had been discharging, I found no trace of either psoas muscle, the place of each muscle being occupied with the cyst of a large abscess; both abscesses communicated above with a space between the bodies of the third and fourth lumbar vertebræ, resulting from the destruction of the intervening fibro-cartilage; but the adjacent bones and the rest of the

fibro-cartilages were perfectly sound." We are indebted to Mr. Stanley for some other observations on this subject having a practical bearing. He has drawn attention to the fact that psoas abscess may occur without any very obvious cause, and unaccompanied by any alteration whatever in the vertebræ or intervertebral substances. He says: "I have happened to examine three such cases, in which no morbid change could be discovered in any of the vertebral structures. Although such cases are rare, the knowledge of them is important, as it instructs us not to regard a psoas abscess as sure evidence of disease of the spine." On the other hand, Sir Benjamin Brodie is very sceptical on this subject, and makes the following remarks, "In systematic works on surgery the lumbar or psoas abscess is usually described as if it were (in some instances at least) a specific disease, having its origin in the psoas muscle. But, according to all the experience which I have had of these cases, this is altogether a mistaken view of the subject. I cannot say that such an abscess never takes place in the loins, but I certainly believe that it is of very rare occurrence. In examining cases of lumbar abscess after death, I have always found caries of the vertebræ in which the abscess manifestly originated. In general the disease of the vertebræ has been so obvious that it could not have been overlooked by the most superficial observer; but in some instances the real nature of the disease has not been detected until after a careful dissection. In one instance, on examining the body of a patient who died in St. George's Hospital with an extensive suppuration in the loins, the soft parts having been entirely removed, not the smallest appearance of disease presented itself in the lumbar vertebræ, and I conceived that I had at last met with an instance of genuine psoas abscess, when almost accidentally a small opening was discovered on one side of the spine in a part which had been covered by one of the attachments of the psoas muscle just large enough to admit a common probe, and forming a communication between the cavity of the abscess and one of the intervertebral spaces. On a further dissection it was ascertained that the intervertebral cartilage had been completely destroyed by ulceration, except at the circumference, and that the opposite surfaces of the bodies of the two contiguous vertebræ were extensively carious." This case is instructive, as it shows the necessity for a very careful scrutiny both of the vertebræ and the intervertebral substances before pronouncing them to be free from disease; but we may reasonably conclude that Mr. Stanley was right in his observations of the three cases of psoas abscess to which he refers as independent of alteration in the vertebral structures. Instances of this form of the disease have occurred in the practice of other

surgeons, and it appears that psoas abscess may be the result of over-exertion and strains. Two or three cases of the kind have been met with by Mr. Bryant. In one patient the affection appeared to be dependent on a strain, which he had sustained in his right groin whilst trying to catch a cricket-ball with his right hand raised above his head. He felt hurt at the time, but went on with the game. The pain continued, and on the second day a swelling appeared in the right groin. This swelling was considered to be an enlargement of the inguinal glands. On the fifth day he was seen by Mr. Bryant. There was some enlargement of the inguinal glands; the thigh was partially flexed, and any attempt at extension caused pain. The pain was increased by pressure made deeply above Poupart's ligament. There was some constitutional disturbance. Four days later, chloroform having been administered for the purpose of relaxing the abdominal muscles, a swelling was detected in the course of the psoas muscle, and bulging of fluid in the groin was produced by pressure made firmly and deeply over the muscle. The fluid projected in the groin to the inner side of the femoral vessels. An incision was made, and about two ounces of pus were evacuated. The wound was dressed with oiled lint. No bad symptoms followed, and in three weeks the patient was well. When seen six weeks subsequently he was in excellent health.

With a view of illustrating some of the remarks which have been already made, I will here relate the chief particulars of some of the cases of psoas abscess which have come under my care at the London Hospital.

Case 1.—Ilio-psoas Abscess, in a middle-aged man, apparently independent of spinal disease.—A. B., forty-five years of age, anæmic and ill-nourished, was admitted into the hospital for pains supposed to be of a rheumatic character, affecting the lower part of the back, and especially in the lumbar hollow. There was no indication whatever of any disease in the spine—not the slightest irregularity or projection of the spinous processes. He could walk well and without pain. His groin was carefully examined, but there was no sign of any swelling; either above or below Poupart's ligament. After the patient had been a short time in the hospital it was observed that he did not stand upright, but leaned his body forwards, inclining it to the right side and flexing the hip-joint. He was able to straighten his body when asked to do so. There was also a marked prominence of the abdomen, accompanied with, and apparently due to, an increase of the posterior lumbar hollow or lordosis. I strongly suspected that there was an abscess in the iliac fossa, and examined him for it, but I could not elicit any fluctuation or feel any tumour through the abdominal

parietes. The patient declined in health, and, if I rightly recollect, succumbed to an attack of diarrhoea. At the post-mortem examination the whole of the fascial investment of the right ilio-psoas muscle was found distended with pus, and the muscle itself pale and atrophied, but not destroyed or rendered incapable of performing its functions. Careful examination of the pelvic bones and the vertebræ by Mr. McCarthy, who conducted the post-mortem, failed in detecting any disease beyond a little roughness and removal of periosteum from the anterior surface of the transverse processes of the lumbar vertebræ on the right side, such as might have arisen as well from the action of the abscess as prior to its formation. There had been no apparent cause for the abscess. The patient had enjoyed fair health, although not long previously he had been an inmate of Guy's Hospital for some affection of the back, perhaps depending on the commencement of the abscess from which he died.

An iliac or psoas abscess may be formed in constitutional conditions of a pyæmic character.

Case 2.—Pyæmic Psoas Abscess.—A young girl was under my care at the hospital suffering from abscess at the back of the right hip-joint. The abscess had been opened and was still discharging from an aperture near the great trochanter. In an interval between my visits to the hospital, the house-surgeon examined the sinus, and thought that he detected a piece of loose bone. When I saw the child again, a day or two afterwards, she was extremely ill, suffering from an attack of peritonitis with symptoms of a pyæmic character. Indeed, I expressed the opinion that the child had pyæmic peritonitis, probably dependent on the previous suppuration around the hip. The house-surgeon was rendering himself anxious about the case, thinking that in examining the sinus he might have explored too far, and interfered with the pelvic peritoneum in the neighbourhood of the sciatic notch. The child died, and the post-mortem examination did not corroborate the fears which had been entertained. The pelvic peritoneum was sound, nor was there a greater amount of inflammation there than in the abdominal region. The cavity of the peritoneum contained a large quantity of sero-purulent fluid, and on stripping up the peritoneum from the iliac fossa the fascial sheath of the left ilio-psoas muscle was found distended with pus. The right hip-joint was quite healthy, the sinus running behind it and in no way communicating with it.

Psoas or ilio-psoas abscess may occur from inflammation of the periosteum covering the vertebræ or the innominate bone, and also in connexion with disease of the sacro-iliac articulation.

Case 3.—About three years ago, a man fifty years of age was admitted under my care at the hospital for some obscure affection of the hip. A careful examination could detect no sign of hip-joint disease, nor any affection of the spine. There was no swelling in the groin. Indeed there were no symptoms whatever, except a complaint of pain down the thigh and disinclination to extend the limb. A long splint was applied to keep the limb extended, but the patient was extremely restless, especially at night, when he became almost delirious, threw off the splint, left his bed, and walked up and down the ward shouting and disturbing all the other patients. About a fortnight after admission, a large swelling suddenly appeared at the upper part of the thigh, extending on both sides and in front of the femoral artery, with evident fluctuation and impulse on coughing. The abscess was opened, a drainage-tube inserted, and the cavity well washed out daily with carbolic acid solution (1 in 40). A very large quantity of the most fetid pus was evacuated. The secretion continued copious day by day. A communication evidently existed with the hip-joint. Hectic supervened, and the patient died. On post-mortem examination the head of the femur was found bare in the acetabulum, and the upper part of the bone was discoloured. The cavity of the abscess extended into the abdomen, and was bounded in front by the sheath of the psoas muscle. There was no disease of the spinal column, but there was ankylosis of the right sacro-iliac articulation, and chronic periostitis of the sacrum and ilium in its neighbourhood. The specimen is in the London Hospital Museum, and shows complete bony ankylosis of the joint, and little spiculated osseous outgrowths in the iliac fossa, near the brim of the pelvis. I have little doubt that the abscess was indirectly caused by the alteration in the sacro-iliac joint. Some years previously the patient had fallen and bruised his right hip and thigh. He was laid up for several weeks, and subsequently to his recovery always retained a limp in his gait. A few months before his admission he suffered from a swelling of the right foot, which ended in abscess. It is not improbable that the ankylosis occurred after the first accident and illness, and that the immediate cause of the abscess was periostitis of the ilium brought on by exposure to cold and privation.

Case 4.—*Psoas Abscess from spinal disease in the lower lumbar region, complicated with pleurisy and pericarditis, and communicating with the hip-joint; death from exhaustion.*—J. D., aged twenty-five, came under my care early in 1873. He had then a swelling in the right groin immediately beneath Poupart's ligament, imparting an impulse on coughing, and yielding the sensation of fluctuation when pressure was made over the iliac

fossa. The femoral artery ran superficially across the swelling, and was felt pulsating immediately beneath the skin and fasciæ. There was no appreciable disease of the spine. The patient could walk and even run without pain; he did not incline his trunk to the right side. The previous history given by the patient pointed to the effects of a sprain. Nine months previously he had started to run, his foot slipped, and for two or three weeks afterwards he experienced pain in his right groin. He recovered, and for some time appeared perfectly well; then he began to suffer from what he thought were rheumatic pains in his hips and numbness in his thighs; a lump appeared in his right groin. Still he could walk well and kick his right leg about without difficulty or pain. He was sent to Brighton, with a view of giving him a chance of absorption of the fluid and improving his general health, and he remained there until the middle of May. He returned in much better bodily condition, but with the abscess rather increased than diminished. It had passed backwards by the side of the small trochanter, and projected in the groin below the fold of the gluteus maximus, forming a swelling there which gave a distinct impulse on coughing, and simulating an abscess or a hernia which had descended through the sciatic foramen. The thigh measured an inch more in circumference than it had done before the patient went to Brighton, and the abscess had descended much lower, reaching beyond the apex of Scarpa's triangle. On the 12th of June, about five months from the time when he first came under observation, the abscess was opened under carbolic acid spray (1 to 20). Fourteen ounces of bland and laudable pus were evacuated. Notwithstanding rigid and complete antiseptic dressing applied to the abscess with the greatest regularity for some weeks, the patient began soon to alter for the worse: hot skin, rapid pulse, anorexia, high temperature with evening exacerbations, were the evidences of hectic or remittent fever; a troublesome cough harassed him, and he wasted daily. It was hoped at first that, as there was no evidence of spinal mischief, the abscess cavity might contract and close, and this hope was strengthened by a temporary amendment which occurred in the patient's condition. The quantity of pus secreted greatly diminished in amount, the abscess cavity contracted, and the opening appeared to be on the point of closing. The appearance of improvement was not maintained; a return of all the unfavourable symptoms occurred; a fresh opening formed near the great trochanter, and not long afterwards bare bone could be felt through the opening; pain, heat, redness, and swelling became observable over the lower part of the abdomen and upper part of the thigh on the left side, threatening the development of a second

abscess. The left thigh was drawn up. These symptoms subsided under treatment. New complications, however, arose. Severe pain over the heart and left side of the chest heralded an attack of pericarditis and pleurisy over a small area in the neighbourhood of the pericardium. Two distinct friction sounds were heard close together, one rapid and synchronising with the heart's action, the other deliberate and synchronising with the respirations. The formation of fresh abscesses in the axilla and over the lumbar spines followed, but notwithstanding all these complications the tenacity of life was so great that the patient lingered for several weeks longer, and died ultimately from exhaustion exactly a year within a day from the time of his first coming under observation.

The post-mortem examination, made under some difficulties at the patient's private residence, revealed the association of spinal disease with the abscess. The intervertebral fibrocartilage between the last lumbar vertebra and the sacrum was in great measure destroyed, the last lumbar vertebra was eroded, and the upper part of the sacrum bare. Both the right and left psoas muscles were destroyed. On the right side the abscess cavity had contracted considerably, but a communication had formed with the hip-joint, destroying the articular cartilage and round ligament, and there was much thickening of the femur with superficial necrosis near the small trochanter. On the left side, the sheath of the ilio-psoas muscle was distended with pus, as far as Poupart's ligament. There was marked amyloid degeneration of the liver, and less advanced change of the same kind in the kidneys. The right lung and pleura were healthy, but the left lung was congested, and there was evidence of recent pleurisy near the pericardium, to which adhesions had been formed. The pericardium was most firmly adherent to the heart on all sides, and the heart appeared to be smaller than usual, as if diminished by the adhesion of the pericardium. There was no valvular disease. The head was not opened.

The points of interest in the case are these:—First, the position of the femoral artery, crossing superficially over the centre of the abscess at the upper part of the thigh. Secondly, the absence of any serious symptoms, either locally or constitutionally, before the abscess was opened, even though it had attained a large size. Thirdly, the failure of the efforts which we made to give the patient a chance of absorption of the abscess. Fourthly, the failure of antiseptic treatment to prevent the accession of hectic fever. Fifthly, the absence of positive evidence, up to a late period of the patient's illness, that the spine was affected. There was no deformity, and no tenderness on pressure or moderate concussion. Sixthly, the

visceral complications, and the long duration of the case—eight months—after the abscess was opened. Seventhly, I may mention the apparent origin of the complaint from a sprain. The patient's father informed us that his son was one of twelve children, all healthy, and born of healthy parents. He had always enjoyed good health. He married early, and had three children at the time of his decease. His father thought his early marriage might have been injurious to him, but attached the greater importance to his fondness for bicycle riding. The complaint had supervened soon after a bicycle journey which he had made to Hertford and back. It is quite likely that the severe exertion thus undergone may have induced the abscess; and it may even be a question whether the abscess followed disease of the spine or the disease of the spine followed the abscess. The remarkable absence of all pain on concussion when the abscess was of large size, the ability to run and stand shocks, rather point to the latter alternative; and this alternative is in accordance with the undoubted after-effects of the abscess in causing necrosis of the femur and ulceration of the articular cartilages of the hip-joint. But I will not undertake to decide the point. Enough has been said to bring the main features of the disease under your notice; and when we next meet I purpose to direct your attention to the important questions of diagnosis and treatment.—*Lancet*, Sept. 26, 1874, p. 441.

ORGANS OF CIRCULATION.

50.—ON THE SURGICAL TREATMENT OF GLUTEAL ANEURISM.

By TIMOTHY HOLMES, Esq., M.A., Professor of Surgery and Pathology to the Royal College of Surgeons.

The artery affected in gluteal aneurism is usually the gluteal itself, but not unfrequently the sciatic. It is impossible, without the opportunity of dissection, to know in any case whether it is the trunk of either artery or one of its branches which is diseased, for both vessels run a very short course undivided. Nor is it by any means easy to distinguish between aneurism of the gluteal and of the sciatic, as Stevens's celebrated case shows. The internal pudic, in its short course over the spine of the ischium, may be affected, as shown by the interesting preparation (Hunterian) which I here produce from our museum. And, finally, there is a curious case reported by Dr. Hilton Fagge, ("Guy's Hospital Reports," 3rd ser., vol. x., 1864,) in which a gluteal aneurism was found to have been developed on an abnormal artery called "sciatico-popliteal." In this

rare abnormality, of which Dr. Fagge's is only the fourth case on record, the internal iliac artery ends in a large vessel which runs down along with the sciatic nerve and becomes popliteal; while the femoral, of very small size, terminates as the *anastomotica magna*.

In Fischer's table of gluteal aneurisms (which, however, includes some cases of wound) thirty-five cases are recorded. Deducting one of aneurism by anastomosis and the above case of Dr. Hilton Fagge, we have thirty-three, in twenty-five of which the affection was supposed to be of the gluteal or its branches, and in six of the sciatic, while in the other two no confident opinion on this point could be formed. This table hardly shows the relative frequency of aneurism of either artery, since the diagnosis was in many cases unverified by dissection; but it seems certain that the gluteal is more frequently the seat both of disease and injury than the sciatica.

The diagnosis of gluteal aneurism has often been found to be extremely difficult. This difficulty has depended, in some cases which are classed as aneurisms, on the absence of pulsation due to the absence of a sac. The majority of these cases were not really aneurisms in any true sense of that term, but wounds or ruptures of the artery; and such cases are, of all others, the most difficult to distinguish, especially when (as frequently happens) the surgeon does not see the case till some days after, when inflammation of the integuments and superficial parts has followed the injury. He has then before him a large swelling, sometimes an enormous one, with decided fluctuation, no pulsation, and inflamed oedematous skin. Unless informed of the history of similar previous cases, he entertains no doubt as to the presence of abscess, and is shocked, on making a free incision into the supposed deep-seated collection of matter, to see a great stream of arterial blood gush out of the wound. This is the history of several of the recorded cases. But there can be no doubt that the disaster might, in some at least, have been avoided by more care in the examination. For it seems certain that in many, if not in all such instances, a bruit might have been detected in some part of the tumour by careful auscultation. And even if there is no distinct arterial lesion, yet many of these great fluctuating tumours of the buttock are mere blood-tumours, which will disappear gradually with rest, if the integumental inflammation be combated by appropriate means. Hence an exploratory puncture, which in no case can do any harm, should never be neglected.

In traumatic aneurism properly so called the diagnosis has also often been erroneous, and that even when the case has been under observation from the beginning; but this also has sometimes depended more on carelessness than on any real difficulty

in making a correct diagnosis. Thus, in Schuh's case, a young man had received a deep wound in the buttock, from which there had been alarming hemorrhage. This was repressed, and the wound healed. After three weeks' stay in hospital, he was dismissed cured, but experienced so much pain on attempting to walk that he returned to the hospital. A fluctuating tumour was found deep in the buttock, and was opened without hesitation as an abscess. A great stream of blood spouted out, and then, after this hemorrhage had been stopped for the time, examination detected (what could certainly have been found out as well or better before) that there were both pulsation and bruit in the tumour. In fact, in the true traumatic aneurisms I should be inclined to believe that the diagnosis from abscess is easier than in some of the spontaneous, since there is the history to guide the surgeon and to lead him to a more careful examination. But I do not mean to deny the real difficulties which in some cases undoubtedly exist. Pirogoff's case is one in point, in which the inflammation of the injured parts led to the diagnosis of threatened abscess, to the application of poultices and mercurial inunction, and finally to a deep incision (but fortunately not deep enough to reach the sac) before the patient's admission into hospital. Then a deeper incision was made under Arendt's direction, but still the sac was not reached. Finally Pirogoff himself repeated the incision, penetrating to the depth of three inches, when a great stream of arterial blood enlightened him as to the nature of the disease; and now, on auscultation, he detected a low dull murmur in one or two places. The history shows how easily that which had led astray three different surgeons, two of them so eminent as Arendt and Pirogoff, might mislead any other surgeon; and it should be added that pulsation was sought for and was proved to be absent.

We see from this the great difficulty of diagnosis in some traumatic cases—those, I imagine, in which the sac is imperfect,—and therefore the absolute necessity of using every method of examination before opening a tumour of this kind.

In the spontaneous form of aneurism other sources of fallacy are added. Here, again, it may easily happen that the integuments have become inflamed either from pressure on the tumour in sitting or in moving, or from applications intended to cure the disease. Then, again, the leading symptoms of the disease are not in general those of aneurism as it occurs in other parts, but loss of motion in the limb, sometimes with persistent flexion, and sciatic pain from pressure on the nerve. Hence the presence of an abscess, either connected with sacro-iliac disease or pressure on the great sciatic nerve, first occurs to the mind of the surgeon. And even if the surgeon can

satisfy himself of the presence of pulsation and bruit, it cannot be denied that pulsation often, and bruit occasionally, are found in those cancerous tumours which are not rarely connected with the pelvic bones.

Let me refer to this diagram from a case operated upon by Uhde, showing the enormous size of the tumour, the several lobes formed by an imperfect sac, and the minute opening by which it communicated with the artery. We may judge how easily such tumours might lose their pulsation from the sac giving way, and how difficult it would be, without very great care, to detect the bruit, which could only be audible close to the entrance of the arterial stream.

The best known instance of mistaken diagnosis is Guthrie's case, the preparation from which is in our museum. In this instance the tumour was taken for a gluteal aneurism by Mr. Guthrie, who saw the case first, although the tumour had not exactly the ordinary features of an aneurism. It is clear from his description that the pulsation was less strong and less expansive than that of an aneurism of so large a size ought to have been. However, there was a bruit. Being in some doubt, he requested the opinion of Sir A. Cooper, Mr. Keate, and Mr. Thomas, who examined the patient separately, leaving their written opinions with the then house-surgeon, Mr. Hancock, and all ultimately coincided in Mr. Guthrie's view, although Mr. Keate was at first more disposed to believe that the tumour was cancerous. Mr. Guthrie attempted to tie the internal iliac artery, but failed to do so, for the tumour extended so high that he could not carry his incision low enough to obtain room, and therefore secured the common iliac, though with much difficulty, and after lacerating the peritoneum in two places. The pulsation in the tumour at once ceased, and the growth temporarily diminished; but it soon became the seat of great pain, and after two months began to grow again rapidly. She died about seven months after the operation.

Mr. Guthrie published the case, had the parts drawn, and presented the preparation to this museum. Therefore it cannot be said that he tried to conceal his error of diagnosis; though, from what he says in his account, it is evident that some one had attributed some such unworthy intention to him; and the eminent opinions which he obtained in favour of the operation would quite acquit him of any blame in the matter. Yet, looking at the case by the light of the experience now acquired, we are entitled at any rate to conjecture that the diagnosis might possibly have been correctly established in spite of the delusive bruit in the tumour.

The case which furnished this preparation to the museum of St. George's Hospital (Ser. ii., 231) is in point here. The

patient was under Mr. Prescott Hewett's care, with a pulsating tumour in the buttock somewhat resembling an aneurism. The absence of bruit in this case, and the fact that pressure in the course of the gluteal artery had no effect on the pulsation, rendered the diagnosis much more easy than in Mr. Guthrie's case; but the same character which is so plainly shown in Mr. Guthrie's preparation was that which in this instance ultimately decided the nature of the disease—namely, that the bones were evidently most extensively affected, so that the pulsating tumour presented on both aspects of the pelvis. It is clear, on reading Guthrie's account and looking at the preparation, that the growth had come into extensive contact with the psoas muscle. Hence, on the assumption that it was an aneurism of the gluteal artery, it must have eroded the pelvic and spinal bones to a very great extent, which would be very improbable in the case of a tumour small originally (as a spontaneous aneurism of the gluteal must be), and having so much more free an access to the soft parts externally. And now that we know how often some amount of bruit is found in these pulsatile tumours of bone, this symptom ought not to have more than its due weight in the diagnosis. Still we must not forget how often, and by what eminent surgeons, the difficulty has been experienced; and we must admit that, in the buttock as elsewhere, the distinction between an aneurism and a pulsatile malignant tumour may easily be mistaken. The latest instance occurred to the celebrated Italian surgeon Professor Porta, who tied the internal iliac artery last year for a supposed gluteal aneurism. The patient died in forty hours from peritonitis, and the disease proved to be malignant. The probability of such an error is another powerful motive for avoiding the ligature of the internal iliac if possible. In Porta's case, also, the bones were extensively affected.

It appears, then, that there are very real and very great difficulties in the way of the diagnosis of aneurisms in the buttock; depending obviously on the great depth at which they are situated, the comparatively small size of the tumour when entire, and the small size of the affected artery. But in many cases it would seem that the tumour has not been entire, but that a part has been ruptured, allowing of the sudden increase of the swelling, and the disappearance of the pulsation. This brings matters very much into the same condition as in a ruptured artery; and the same care is necessary to auscultate the part, and, in case the doubt is not thereby cleared up, to use the exploring needle. More especially should a minute and careful examination be made of the bones of the spine and pelvis, under chloroform if necessary, both from the surface of the body in front and behind and from the rectum.

Let us now see what the experience of surgeons has been in the treatment of aneurisms in the buttock considered to be of spontaneous origin.

In these cases there is at any rate no certainty whereabouts the opening of the sac may be, nor what may be the condition of the coats of the vessel at the affected part. Hence it is not to be wondered at that the treatment by Hunter's operation has been far more generally carried out.

The number of cases of so-called spontaneous gluteal aneurism in Fischer's table is 21. Of these I should be disposed to reject one (Baun's case), which seems to have been an erectile tumour, or teleangiectasis, and to supply the one by Thomas which I previously quoted. This leaves still 21 cases, in 5 of which no treatment was adopted. Out of the other 16, 9 were treated by ligature of the internal, and 2 by ligature of the common iliac, 4 by injection of the perchloride of iron (in one of these cases compression of the aorta had been tried and failed), and in the other case cure is attributed to rest and diet, assisted by direct pressure.

If we try to form a general opinion as to the treatment of gluteal aneurism, we shall find both that in this, as in all other aneurisms, the doctrines of the surgeons of the present day vary much from those of our immediate predecessors, and also that the special difficulties of the treatment in this region give rise to much difference of opinion.

Mr. Guthrie, when lecturing on the subject of aneurism at this College, forty-five years ago, laid it down as if it were incontestable, that "in all cases of aneurism of the gluteal and sciatic arteries the internal iliac artery should be tied, instead of an operation on the part itself;" while Mr. Syme, even after a successful operation on the internal iliac for gluteal aneurism, expressed his preference for the old operation in any case in which he could be confident of finding the orifice of the sac external to the pelvis. M. Buisson, on the other hand, is so impressed with the danger of the operation on the internal iliac, the frequency of the anomalies of that artery, and the proximity in all cases of the ligature to the large branches and to the sac, that he gives a preference to the method of Anel—*i.e.*, to the ligature of the affected artery as it reaches the tumour. Servier, again, gives the preference to the injection of the perchloride of iron; and in this he is followed by Fischer, who, after a long review of all the opinions of his predecessors, pronounces the conclusion, that "the injection of perchloride of iron must be looked on as the best treatment for gluteal aneurism."

But we must allow that every one of these measures is fraught with danger, and that some of them are very likely to

be mechanically impossible. The ligature on the Hunterian principle can indeed always be carried out, but as applied to the common iliac it has never succeeded; and since the ligature of the internal has certainly, as far as present experience goes, proved less fatal than that of the common iliac, I think the latter operation should be left out of consideration, as being not indicated. But the ligature of the internal iliac artery for gluteal aneurism has hitherto proved fatal in half the cases operated on, and it is really doubtful whether in many cases of the disease the urgency of the symptoms and the danger to life are sufficient to justify so very grave a risk. We must not forget that neither this nor any other form of aneurism, when of moderate size, is insusceptible of spontaneous cure. For instance, M. Bouisson describes and figures a preparation removed from the body of an elderly woman brought to the dissecting-room, showing an old aneurism of the gluteal artery soundly cured. He relates also the case of a medical man, a patient of M. Dubrueil, who had "a voluminous aneurismal tumour, developed spontaneously in the course of the gluteal artery, which, after having acquired sufficient volume to render all its characteristics well marked, had remained stationary for many years. The patient had declined all operative interference." Again, in the case of traumatic aneurism which Mr. Syme treated by laying the sac open, he records that the wound had been received seven years before, and had resulted in the formation of a pulsating tumour, which remained during the whole period, having apparently been developed at once; but "this had occasioned little inconvenience, and rather been a subject of amusement to himself and friends until lately, when it suddenly enlarged and became the source of pain." The probability is that, if this man, instead of amusing himself and friends with so dangerous a plaything, had taken all imaginable care of himself, this rapid growth of the sac, depending, as it seems, on some partial rupture, would not have occurred. I cannot say that, for my own part, I should dissent from the decision of M. Dubrueil's medical patient; or that, except under very unusual circumstances, I should think myself justified in recommending the ligature of the internal iliac. Those circumstances I will endeavour to specify presently.

M. Bouisson, whose opinion is always entitled to the greatest respect, does not, in treating of aneurisms in the buttock, appear to me to have looked sufficiently narrowly at the various conditions in which such aneurisms may be found. And he appears to have been almost too much impressed with the dangers and difficulties of the Hunterian ligature, and with the ease with which he succeeded in securing the gluteal in an open wound. A more extended experience than was at that time

perhaps attainable has shown that the operation of Anel is not always, perhaps not often possible; and that even in traumatic aneurisms the wound in the vessel may have been inside the pelvis, or possibly the wounded part of the artery may have been pushed into the pelvis by the growth of the sac. Such was the case in Schuh's patient, where, even after the sac had been laid open, it was found necessary to plunge the aneurism needle into the pelvis, embracing the nerve as well as the vessels, before the hemorrhage could be stopped. And even in Bouisson's own case, since the artery was divided close to the margin of the sciatic notch, it is hard to see how Anel's operation could have been carried out, supposing that the artery had not been at once secured and that a traumatic aneurism had formed. Again, the experience which has been obtained of ligature of the artery close to the sac of a gluteal aneurism is not very encouraging. Of three cases—Campbell's, Dugas's, and Sappey's—only one was successful, and that in a traumatic case occurring in childhood. It is, however, a feasible proposal in cases such as Sappey's, where the aneurism is situated some distance from the pelvis, and especially where, as in that case, the artery leading to it can be commanded by pressure from the skin. But the proposal should not, I think, be entertained till after the failure of milder measures. I ought not to omit that M. Sappey himself concluded from the failure of his case that the method is not suited to sciatic aneurism, on account of the free anastomoses carried on by the branches of that artery; but the experience of only a single case seems too little to justify any general conclusion of this sort.

There is another alleged case of spontaneous cure of gluteal aneurism related by Dr. Stoker in the *Transactions of the King and Queen's College of Physicians of Ireland*, vol. i., 1817, p. 41. This is the case variously attributed by most writers on the subject to Crampton and Stokes. It must be admitted that this case would have been more satisfactory had there been more particulars of the symptoms and history. All that we are told is, that Mr. Richards, who was in attendance on the gentleman, a man aged thirty-four, for some chest affection (accompanied by palpitation of the heart and increased vascular action), discovered a tumour, which Dr. Stoker found on examination to be an aneurism of the gluteal artery; that the patient refused to have a consultation on the case; that Mr. Richards had previously applied a compress over the aneurism for several weeks, which had prevented its further extension, but without diminishing its size; that a light vegetable diet, with gentle laxatives, was ordered under Dr. Stoker's direction, and digitalis in pills "in such quantity as to produce a decided effect in moderating the great and general vascular action,"

and that then "the tumour soon began to diminish very sensibly under the continued pressure, and in less than fourteen days was entirely removed, nor has there been any return of the general or local symptoms." This was fourteen years before.

I allow that a fuller account of this case ought to have been given in order to produce a conviction of its accuracy; still there is nothing incredible in it, and, in spite of Samuel Cooper's rejection of it, it has been accepted as true by Fischer and other critics, and I am myself disposed to believe in it, Bouisson's case showing the perfect possibility of the occurrence.

The old operation, by laying open the sac and tying the vessel, is a desperate business, and in spite of the really considerable amount of success which has attended it, no prudent surgeon could contemplate it without repugnance. John Bell's description has often been reflected on as theatrical and exaggerated; but it is questionable whether it really deserves to be so regarded. The accounts left by Carmichael, Syme, and Schuh, show that even in smaller tumours than the one which Bell was dealing with, the patient runs great risk of bleeding to death, and the surgeon has need of all his dexterity and presence of mind to save him; while in Toracchi's case the operation proved impossible, the wounded artery having retracted into the pelvis. This is indeed the most formidable danger in this operation—one which was foreseen by the sagacity of Mr. Syme, though he does not seem to have known of Toracchi's case—and which should be materially weighed by any surgeon who proposes to undertake this somewhat desperate operation. In spontaneous aneurism there is probably more risk of such a catastrophe than in traumatic, but the old operation has hitherto been applied in traumatic cases. I do not know that there is any valid reason for this restriction, for the reasoning of Bouisson is here very cogent. The ligature, if placed on the internal iliac artery, is too near the sac to allow the patient the full value of Hunter's proposal, as Uhde's case conclusively proves, and if the old operation be less dangerous than the ligature of the internal iliac, there is no reason, I think, why it should not be practised in spontaneous equally as in traumatic aneurisms.

But it seems necessary to examine the tumour very carefully beforehand, in order to determine the prospect that there is of finding the artery outside the pelvis. In some cases indeed (as in that of Sappey), there is no doubt on this point: the tumour can be drawn away from the bone; and even, perhaps, as in that case, the artery leading to it can be commanded by pressure above the tumour. But when the pulsating tumour reaches up to the pelvis, it is necessary to examine the part very carefully from the rectum, or, in the female, from the rectum

and vagina, in order if possible to fix the level of the mouth of the sac. Now that surgeons have ascertained the possibility of passing the whole hand into the rectum, under chloroform, without injury to the functions of the bowel, much more exact ideas on this head may be formed than were formerly possible. If any considerable part of the tumour lay within the pelvis, I would certainly not risk the old operation; although in Bigelow's case, where a part or loculus of the sac encroached on the pelvis, the orifice of the artery was outside.

This diagram, from Uhde's case of gluteal aneurism, shows how very difficult it may be to find the arterial orifice after laying the sac open. In the enormous tumour here figured, extending from the buttock below the middle of the thigh, the arterial orifice appeared at first sight as a mere pinhole, and even after drawing a coagulum out of it would only admit a probe.

I cannot but think that those who have spoken so favourably of coagulating injections in gluteal aneurism have hardly weighed very accurately the evidence of the published cases. I agree with Fischer that the evidence against coagulating injections in this disease is very untrustworthy—for too little is known of Bruns's case to enable us to judge how far the treatment was to blame for its fatal event; and in Legouest's case (related by Servier), an injudicious quantity of the solution seems to have been injected, besides that the case appears to have been in itself well nigh incurable. But then the evidence of success is equally wanting. In Campbell's case the injection failed, on a single trial it is true. In Baum's case of true aneurism (for the case of aneurism by anastomosis has no bearing on the question) it is quite evident that it failed also, and the aneurism was cured spontaneously more than two years afterwards. There remains only Nélaton's case (the one originally operated on by Sappey), and there, though the account leaves the patient in a very favourable condition, there is really no proof of definite and permanent cure.

Not that I am opposed to the trial of coagulating injections in gluteal aneurism; on the contrary, I advocate it.

We have no experience as yet of the two methods of treatment to which I am disposed to look most hopefully for the cure of these aneurisms—viz., compression of the common iliac artery (or aorta) and galvano-puncture. It is true that Legouest tried the former, and Blasius the latter; but it is only necessary to read the history of Blasius's case to see that the application was not efficient; and Legouest's case was one which probably could not have been cured by compression under any circumstances; nor was the compression very perfectly or methodically carried out. Now, gluteal aneurism

presents almost every feature which is encouraging for compression. The tumour, if the sac has not burst, is usually of no great size, it does not encroach on the abdomen, and any part of the aorta or common iliac is accessible to pressure. Surely the first step in the treatment of such a case should be to try the effect of methodical compression of the aorta or common iliac artery. If this can be done successfully without chloroform, which in a case not very acute might, I think, be possible, the patient would not be exposed to any serious danger; and we have too many histories now of aneurisms, even of large size, being cured by intermittent pressure, to deny the possibility; while, on the other hand, the history of such cases as I have referred to, in Dubrueil's and Syme's patients, proves that it is not every case of gluteal aneurism which justifies dangerous measures. If compression without chloroform is not possible, then the cure of the aneurism by total compression under chloroform should be attempted, and will in all probability be obtained in a large proportion of cases. If it fail, it may be repeated, in combination with coagulating injection, or galvano-puncture may be, and should be, tried. It is only after the failure of all these measures that the question of a capital operation occurs, and that question is no doubt a very serious one. I confess that I should be disposed to restrict the operation on the internal iliac artery to those cases in which there seem to be grave doubts whether the mouth of the aneurism is not inside the pelvis; whenever it is plainly outside that cavity I would perform Anel's operation, and if on trial this should prove impossible, I would open the sac. It must be recollected that the efficient pressure which the abdominal tourniquet now enables us to apply to the common iliac artery would rob the old operation of much of its danger.

There are only two cases of antero-venous aneurism in the buttock on record, and in both the sciatic was the artery affected. At least this seems more probable, but there is some discrepancy between various accounts of Nélaton's case as to whether it was of the gluteal or the sciatic vessels. Nélaton's patient was cured by the injection of perchloride of iron. Ribéri's derived benefit from compression, but was not cured. The example of this case of Nélaton's would encourage a surgeon to attempt the cure of this disease by injection of perchloride of iron, or perhaps by galvano-puncture with the abdominal tourniquet; but on the failure of such attempts, the old operation would be the only trustworthy method, if the symptoms were sufficiently urgent to justify the risk.

The conclusions, then, to which present experience seems to me to point in the treatment of gluteal aneurism are as follows:—

1. Gluteal aneurisms, both traumatic and spontaneous, are very favourably circumstanced for the treatment by either rapid or gradual compression, applied to the aorta or common iliac.

2. If this treatment does not succeed by itself, it may be supplemented by coagulating injection or galvano-puncture, performed while the patient is narcotised, and the circulation commanded.

3. When such treatment fails, and particularly in aneurisms with imperfect or ruptured sacs where it is not indicated, the internal iliac must be tied when the surgeon thinks that he cannot find the artery outside the pelvis. But when the artery is accessible, the old operation, or the operation of Anel, should be practised, according to the size and extent of the tumour.

4. The ligature of the internal iliac artery is liable to failure in cases of spontaneous aneurism from a diseased condition of the coats of the artery, and should always be avoided when other means of treatment are available.—*Lancet*, July 11 and 18, 1874, pp. 37, 74.

51.—ON THE SURGICAL TREATMENT OF INGUINAL AND FEMORAL ANEURISM.

By TIMOTHY HOLMES, Esq., M.A., Professor of Surgery and Pathology to the Royal College of Surgeons.

The experience of our British hospitals has been, on the whole, favourable in the treatment of this form of aneurism by cutting operations, whether in unruptured aneurisms, after the method of Hunter, applied to the external iliac artery or the femoral, or in ruptured aneurisms, by tying both ends of the vessel, after the method of Antyllus; whilst the experience of pressure, so far as it has gone, has been less favourable, and offers a very great contrast to the account given by Fischer of the cases to be found in published literature.

Two causes may be imagined for this contrast. It may either be assumed (for we have no proof either way) that our hospital patients are less tolerant of pressure or that their cases are less fitted for the treatment than those in general practice. And there is no doubt that many of our patients are irritable from drunkenness, and that in many cases they have neglected to take advice till the growth of the tumour has rendered it little amenable to compression.

But it is also possible that the method of compression may not be followed out in our hospitals with that minute care and that attention to every detail of the case which are really essential to its successful application. The treatment has not a fair

chance if carried on by persons who are not responsible for its success, and who may be hardly interested in the event of the case, with just a hasty visit once or twice a day from the superintending surgeon. The compression treatment is far more laborious for the surgeon in charge of the case than the operative treatment; and I confess to a suspicion that, if hospital practice is to be carried on only by daily visits, the treatment of cases of aneurism in the lower extremity will be more successfully conducted on the Hunterian method than on that which is intended to replace it. At the same time, this accidental circumstance affords no indication of the real value of the two methods of treatment.

My own opinion, derived from an extensive search into the published records of practice and from a tolerably long experience of the results of treatment in our hospitals, is that the treatment of aneurism by operation in the latter is, on the whole, eminently successful. At the same time, I believe that this success might be even further extended if the treatment by compression, and particularly by digital pressure, were more completely and more systematically carried out than it is at present, and if hospital surgeons had sufficient confidence and sufficient interest in the method to sacrifice to it the time and attention which are necessary in order to obtain from it the best results which it is capable of giving.

I am speaking before an audience composed in great part of hospital surgeons, and they can judge whether what I say has any likelihood or importance.

Another striking fact in this table is the sharp contrast which it shows between the results of rapid and total pressure under chloroform, which as a rule has been very successful, and those of the more gradual method by instrumental or digital pressure, which, in femoral aneurism at any rate, has been very much the reverse of successful in the hands of British hospital surgeons; and this will, I think, be a convenient place to exhibit, as far as present experience enables me to do so, the results which have actually been obtained by this novel method, not only in femoral but in other forms of aneurism below the diaphragm to which it has been applied.

The very successful case of abdominal aneurism which Dr. Murray published some years ago, where a cure was effected by rapid compression of the abdominal aorta under chloroform, was such a real advance in the cure of a disease previously regarded as inevitably fatal, that it has led to very general imitation, and numerous similar cases have been recorded. Some surgeons have gone so far as to recommend this method of rapid pressure under chloroform applied to one of the large abdominal trunks, as superior to the more gradual method

applied to the femoral artery in the groin, for the cure of femoral and popliteal aneurisms. No doubt the circulation in the thigh and leg may be powerfully, even alarmingly, affected by compression of the aorta or common iliac. This is attested by the coldness and lividity of the lower extremities which have been noticed during such compression, and which have persisted for a length of time that rendered gangrene very probable.

We are not, I think, as yet in a position to give any other than a conjectural opinion as to the desirability and the chances of success of the rapid method of pressure, applied under anæsthesia, in situations like the popliteal, where the other plans are feasible. Very few cases, indeed, have occurred in which this method has been applied to the femoral artery for the cure of popliteal aneurism; and that for the very satisfactory reason that patients can usually tolerate effectual pressure on the femoral without chloroform. But, in cases of femoral aneurism, it is often necessary, if compression is used at all, that it should be used in the form of the rapid and total compression of the external iliac, under chloroform or ether, since the patient cannot bear the amount of force required without an anæsthetic; and it is one of the most interesting and most difficult questions in the surgery of such aneurisms to determine whether this treatment is really more or less dangerous than the resort to the Hunterian operation on the external iliac artery. The number of cases which have as yet been under treatment cannot be large; and, perhaps, I have not succeeded in finding all those that have been published. It is a singular fact that all these cases are either English or American. Some cases I should have thought must have occurred on the Continent; but, if they have been recorded, I have missed them. But I will endeavour in this place to put on record those which I have met with in books, or which have been kindly communicated to me from the various hospitals, in order to exhibit, as far as I can, a correct picture of the benefits and the risks connected with this treatment. It is evident enough, from the way in which "the rapid method" is sometimes spoken of, that the few striking cases of cure which have been published have attracted more attention than the instances of failure, although great candour has been shown in putting these on record, so far as they have been deemed worthy of publication. I have previously called attention to the risks which attend the rapid method of pressure applied to the abdominal aorta—risks which, as I before explained, do not justify the surgeon in refusing to his patient the prospect of cure which this operation holds out, and which very probably are not greater than those which are inseparable from

all grave surgical operations; but those risks certainly ought to be clearly known to any surgeon who proposes to undertake the operation, and deliberately weighed by him against the apparent dangers of the disease, and against the risks of any other method by which it may be treated.

A very interesting case appears to me to show the occasional utility of manipulation in aneurisms as suggested by Sir Wm. Fergusson. It was under the care of Mr. Herbert Page, in the Cumberland Infirmary at Carlisle, in the year 1872. The patient was a railway guard, aged twenty-seven, who had had syphilis, but in whom there was no known cause for the aneurism. He had been labouring under aneurism of the left femoral artery, in the upper part of Hunter's canal, for three weeks before admission; and about a fortnight after admission, a similar tumour showed itself in the same situation on the other side. Instrumental compression was tried on the left side, and was well borne for a few days, but it soon became intolerable; the tumour began to grow rapidly, with great pain, and the artery was tied. All went on well on this side, except that recovery was somewhat retarded by deep-seated suppuration in the wound. During his convalescence from the ligature on the left side, the right aneurism at first almost disappeared; but in about a month it began to grow again, with much pain both in the tumour and in the leg. He could not tolerate any compression on the artery above the tumour. On Sept. 11th and 12th (the wound made for the ligature of the left artery being then all but closed), Mr. Herbert Page manipulated the tumour, with the view suggested by Mr. Oliver Pemberton's Address in Surgery at the British Medical Association about that time—viz., "to alter the relations of the laminated fibrin in the cavity of the aneurism, so as to bring about a further deposition of fibrin on the projecting surfaces of the displaced laminæ." The pain in the swelling and in the leg was evidently severe. He could not sleep at night; and on Sept. 14th could hardly bear to be touched, the parts were so tender. However, next day (Sept. 15th) it was noted that there was no pulsation, and that the pain had gone; but some circulation seemed still going on through the tumour, since it diminished in size on compression of the artery, and increased again when the circulation was restored. From this time the tumour kept on diminishing in size and increasing in solidity; and in sixteen days from the time of manipulation the man was well enough to go out in a Bath-chair. He was discharged on Oct. 2nd, there being then a small, hard nodule, the remains of the aneurisms, in each thigh. But when seen again in December no trace of either tumour could be detected on the most careful examination.

It is certainly open to anyone to contend that this was a case of spontaneous cure ; but as the aneurism was increasing rapidly up to the date of the manipulations, as those manipulations produced a most decided effect, and as they were followed by the precise train of phenomena which were sought to be obtained and which led immediately to the consolidation of the aneurism, I have no hesitation myself in regarding this as a case of cure by manipulation ; and I may say that, having shown the notes to Sir W. Fergusson, he is of the same opinion.

One of the most interesting cases of aneurism of the external iliac on record is that which was published in Mr. Oliver Pemberton's Address in Surgery, to illustrate the use of direct pressure in the cure of aneurism. The patient was a healthy active man, aged sixty-seven, who had an aneurism of recent formation just above Poupart's ligament. Seeing the danger of a ligature to the common iliac, which was at that time (1859) the only known alternative, Mr. Pemberton tried the effect of pressure on the artery above, but found that this did not arrest the circulation, which, however, he noticed was much retarded by pressure on the sac itself. Accordingly, Carte's compressor was carefully applied to the sac itself, under the management of two senior students, with only sufficient force to retard the circulation, and for not more than three or four hours at a time—an average of about seven hours a day. For a fortnight no good was done. During the next eight days a good deal of coagulation was procured. He was then allowed a rest of nine days, and permitted to move about the room. Then followed nine days of treatment, averaging six and a half hours a day ; and from that time the aneurism ceased to pulsate. When Mr. Pemberton published his Address the man was still alive, active, and well, at the age of eighty-three. The remains of the aneurism were represented by an indurated enlargement the size of a chesnut.

With this I would connect an interesting case of which Mr. Gant has shown me the notes. The patient was very old (seventy-five), with a recent aneurism in the groin, the result of a fall. Pressure above being impossible and ligature hopeless, a Carte's tourniquet was adjusted just below the aneurism, and applied for about two hours daily during about ten weeks. This caused much pain, extending also to the aneurism ; but the latter decreased in size and pulsation, and became practically cured. He was seen six months afterwards, the tumour being then very small, with some pulsation, but not of an expansive character.

Arterio-venous aneurism of the femoral artery and vein is not, on the whole, an exceedingly uncommon affection ; at least, there are a good many examples on record, spontaneous

as well as traumatic, though the latter are, of course, by far the most numerous. Our own museum contains a splendid specimen of this disease, which was given to it by Mr. Beaumont, of Canada (No. 1570 C), and the very interesting history of which is contained in the *Medical Times and Gazette* of July 17th, 1867. It is well worthy of deliberate study as containing many very singular features, but time does not permit me to dwell on them fully. Suffice it to say that the communication between the artery and vein would seem to have persisted for ten years and a half (after a wound followed by very alarming hemorrhage) before any tumour formed; and the evidence of this was that during the whole of that period the peculiar bruit had been audible, not only to the patient, but to those about him. The cause was a stab received in a quarrel, and the bleeding had been so severe that for two days the patient was near to death. It was only after this long period of abeyance that an aneurismal sac seems to have formed in consequence of some strain while on horseback, which probably caused the giving way of the weakened portion of the arterial wall. The patient died under the influence of chloroform, administered with a view to the ligature of the external iliac artery, and this operation was rendered necessary by the rapid growth of the tumour. Yet, as Mr. Beaumont remarks, the state of parts which the dissection reveals renders it very improbable that the operation could have succeeded, for the superficial femoral vein is altogether blocked above and below the communication with the artery, and the profunda artery and vein are so far compressed by the tumour that they could transmit little if any blood. Besides, in this, as in most cases of arterio-venous aneurism, the artery above the tumour is weakened, dilated, and thrown into curves, so that here the external iliac measured five inches in length. The same was the case in Mr. Perry's patient, where the external iliac was so folded on itself as to form a pulsating tumour, and give rise to the suspicion of another aneurism there. Hence, if the operation had been completed, it seems certain that the patient must have succumbed either to secondary hemorrhage or to gangrene. Otherwise the obliteration of the femoral vein above and below the opening would in itself have been a favourable feature in the case, reducing the arterio-venous aneurism more nearly to the conditions of an ordinary arterial aneurism. Yet it may be doubted, from the notes of the case, how far this obstruction could have been perfect during life, since the venous murmur and thrill seem to have been perceptible to the end.

Before quitting this case, let me remark on two of its features. One is that in this, as in Mr. Perry's case of arterio-venous aneurism in the thigh, the sac was ossified. This may

be a matter more of curiosity than of practical interest. But it is certainly important in a practical point of view to note that, with the femoral vein occupied by coagula, and with the profunda vein so compressed as hardly to transmit any blood, the anastomosing veins, with the saphena, had so completely carried on the circulation, that there was not only no gangrene, but no œdema of the limb. Surely this is a fact not to be overlooked in considering the propriety, in circumstances otherwise desperate, of tying up the artery and the vein together in operations for arterio-venous aneurism.

There are many other cases of arterio-venous aneurism in the thigh to which I should like, if time permitted it, to call your attention in detail. One of the leading cases in this particular is Perry's, where, without any known cause, an arterio-venous aneurism formed in the thigh of a middle-aged man who had been suffering from an aneurism at the bifurcation of the popliteal artery of the same side for about two years. I shall presently dwell on some of the interesting features of this case, which, however, will well repay a more careful study.

But of all cases which are reported in surgical literature, the one which is most interesting and which leads to the widest speculations as to the pathology and treatment of aneurism, is Mr. Oliver Pemberton's. In that case the patient had suffered from spontaneous aneurism of the posterior tibial artery, in itself a rare event. This had been cured by the use of compression by means of a tourniquet to the femoral artery in the groin, continued at intervals for nine months, and sometimes applied with considerable severity, till the patient could bear it no longer. The femoral artery on both sides, it should be noticed, had been observed beforehand to be "hard and cord-like." Nine or ten months after the cure of this aneurism, an arterio-venous aneurism showed itself in the groin, at the seat of pressure, and this went on increasing till his death, which occurred twenty months afterwards, as the result of dropsy dependent on disease of the heart and liver. During this time the disease had made progress, the veins had become large and tortuous, and the extremity much enlarged. It was evidently the condition of the viscera which forbade the attempt at any cure of the disease, and doubtless in this case such contra-indication was complete.

I am embarrassed by the numerous points of interest which this well-known case comprises, and which have reference, first, to the pathology of the disease; secondly, to its diagnosis; and, thirdly, to its treatment. I had better perhaps, leave aside the first point to be treated hereafter, when we come to talk about the possible ill-consequences of compression applied for a long time to arteries not perfectly healthy.

I quite admit the possibility, even the probability, of Mr. Pemberton's supposition, that the communication between the artery and the vein in this case arose in consequence of some chronic action in the walls of the artery set up by the long-continued pressure, though neither the author nor anyone else could say that such a cause has been proved, especially when we consider the many points of resemblance which Perry's case, indubitably spontaneous, bears to this. As to the diagnosis, I must limit myself to saying that it was clearly and correctly established by noticing the continuous harsh murmur, "like the ceaseless rushing movements of the steam forge-hammer," and "so harsh that it becomes acutely disagreeable, to retain the stethoscope for more than a few moments," which was traceable from one special point of the tumour, and by the enlargement of all the veins leading to that point from above and below. It would have been useless to speak of treatment in a case like this, where the patient was dying of visceral disease, and where the aneurismal tumour was really only a subordinate part of his sufferings. But none the less is the case very instructive, especially taken in connexion with Mr. Perry's, in showing at any rate what ought to be avoided. In both there was a small tumour, causing much venous enlargement and œdema, and a remarkable dilatation, thinning, and elongation of the artery above its point of communication with the vein—a circumstance which has been often noticed in arterio-venous aneurism.

I think the obvious inferences derived from a study of these and other similar cases would be two.

First. That the treatment must be directed to obliterate the orifice of communication between the artery and the vein. It is from this communication that all the secondary consequences of the affection seem to proceed—viz., varicosity of the veins, degeneration of the artery, œdema, pain, and loss of the functions of the limb. This communication may be obliterated in three ways:—1. By laying the sac open, and tying the artery above and below the wound. When this has been done the surgeon may use his own judgment as to tying the vein also. 2. By tying the artery above and below outside the sac. 3. By compression applied to the venous orifice and to the artery above the tumour. This is best done simultaneously after Vanzetti's method. But cases have been successfully treated by first compressing the venous orifice until the latter has been obstructed by coagula; the arterio-venous is thus converted into a simple arterial aneurism, which is afterwards treated in the usual manner.

Secondly. All cases which have been dissected at a late stage of the disease prove that the artery becomes so attenuated

and enlarged above the tumour that no operative interference can then be successful. Hence the necessity for treating the disease decisively at first, and for abandoning operative interference if the case is not seen sufficiently early.

On a review of the whole subject of femoral aneurism by the light of our present experience, the following are the main conclusions to which Professor Holmes is led :—

1. That the operation of ligature of the external iliac artery has been, on the whole, fairly successful, as evidenced by a very small mortality in uncomplicated cases of hemorrhage and a mortality of about one-fourth in published cases of aneurism—a conclusion supported by the unpublished records of hospital practice, though a few cases of recurrence of the aneurism have occurred.

2. That the operation on the superficial femoral, for aneurism situated in Hunter's canal, is a very successful operation.

3. That the ligature of the common femoral is a perfectly justifiable proceeding; though whether more or less trustworthy than that of the external iliac artery we are not as yet in a position to judge.

4. That ruptured aneurism in the thigh has been treated with a large amount of success by the old operation.

5. That ilio-femoral and femoral aneurisms have been treated with a very fair proportion of cures in the few instances on record by rapid compression applied to the aorta or common iliac, but that there is no evidence to show that this treatment is less dangerous or more successful than the operation on the external iliac artery, when the latter is feasible.

6. That compression, especially digital pressure, has been applied to the treatment of inguinal and femoral aneurism with striking success, though in what proportion of cases we do not as yet know. That the comparative ill-success of this method in our hospital practice is more calculated to raise doubts of the efficiency of the application than of the soundness of the method itself.

7. That in rare cases direct pressure or even manipulation may be advantageous.

8. That arterio-venous femoral aneurism should be treated by double compression, applied to the vein and artery; which failing, Mr. Spence's method of tying the artery above and below is the most hopeful measure; and when this is impracticable, either the old operation should be preferred or the case abandoned.

9. That spontaneous aneurisms of the profunda have been diagnosed and successfully treated by compression.

10. That recent traumatic aneurisms of branches of the external iliac or femoral are best treated as wounds of these

vessels—*i.e.*, either by compression or by ligature at the wounded part.—*Lancet*, Oct. 10, 1874, p. 507, and *Medical Times and Gazette*, July 11, 1874, p. 32.

52.—ON THE TREATMENT OF POPLITEAL ANEURISM.

By TIMOTHY HOLMES, Esq., M.A., Professor of Surgery and Pathology to the Royal College of Surgeons.

The Professor commenced his remarks on the surgical treatment of popliteal aneurism by repeating the observations which he formerly made on other kinds of aneurism as to the great importance of the composition of the sac.

1. It is delusive to expect that equally mild measures will succeed in a rapidly growing aneurism when the sac is thin and probably imperfect—as in one in which the sac contains a considerable proportion of the firm, elastic tissue derived either from the expanded arterial wall or from well-formed and consolidated areolar membrane. At an early period of the formation of an aneurism resulting from injury, we see very clearly the slight resistance which the surrounding tissues will oppose to its increase—allowing that the rupture of the coats is complete,—as contrasted with the difficulty experienced by the artery in the manufacture of the aneurismal sac when only a portion of the coats has given way. This difference is clearly pointed out by John Hunter in his remarks on the preparations of traumatic popliteal aneurism (No. 1571 in the Hunterian Museum). Hunter says—“Here was a case where there was every external appearance of an aneurism, such as a circumscribed swelling with a pulsation. This was what would be called or understood by a spurious aneurism, but it was properly a rupture of the coats of the artery, and which I do imagine is only to be distinguished from the aneurism or dilatation by the time it takes in coming to its ultimate size—*viz.*, from its first appearance to its threatening destruction to the parts beyond, as a limb, or destruction in the surrounding parts in which it is placed—threatens, *viz.*, mortification and bursting; an aneurism being as many months in coming to this ultimate as this disease was days; for in aneurism, although the artery gives way at last, and then its coats are principally composed of the condensed cellular membrane (as in this case), yet it is strong, owing to the time it has had to thicken and form a coat while the artery was dilating.”

The difference pointed out by Hunter, and inferred by him from this pathological specimen, is often very clearly marked in practice. It is true that in cases of extensive or total laceration of the wall of an artery no sac is usually formed.

The blood is extravasated into the cellular membranes, but there is no pulsation—and in these cases there will most likely be no aneurism at all,—the blood extends indefinitely, the limb will mortify, and amputation must be performed. Such cases may be likened to those of extravasation of urine from free rupture of the urethra, infiltrating the areolar tissue to an indefinite distance.

But there are other cases, more like those of perineal abscess following on a slight ulceration of a portion of the urethra behind a stricture where there is a small laceration of all the coats of the vessel and a limited quantity of blood is extravasated. In these cases a sac is formed sometimes with amazing rapidity, and grows as rapidly as it forms; most interesting example is one published by Mr. G. E. Walker, of Liverpool, in the Liverpool Medical and Surgical Reports, vol. v. There are many somewhat similar cases which show the failure of pressure, or the great difficulty in bringing it to a successful issue in cases of traumatic aneurism, or of aneurism following partial rupture of an artery, where the sac is very thin or imperfect. It appears to Mr. Holmes that both theoretical and practical considerations point to the conclusion that for the speedy and secure consolidation of an aneurism the concurrence of the contracting action of the sac is almost as important as the coagulation of the blood within it. And if this be so, it accounts for the great difficulty in procuring a stable cure by any method such as galvano-puncture, which is intended to act upon the blood alone—and still more so by the introduction of foreign bodies into the tumour, with which they may consolidate the blood; these must certainly irritate and soften to some extent the tissues of the sac.

2. Besides the differences which exist between one aneurism and another in respect to the composition of the sac, there is a very important difference in popliteal aneurism in respect of its position—*i.e.*, whether the orifice of the aneurism is on the deep face of the artery, and the tumour grows towards the knee-joint, lifting up the vessel; or, on the other hand, is situated at the back of the artery, growing towards the vein, the nerve, and the skin; or laterally; or is of the fusiform or tubular variety, in which case it often extends up Hunter's canal and becomes femoro-popliteal.

It is quite evident that such differences in form and in anatomical relations must powerfully influence both the course of the symptoms and the effect of treatment. For instance, the aneurisms which grow towards the knee-joint must produce more difficulty and stiffness in the motions of the knee; they must be more liable to be accompanied by synovial effusion, and sometimes by more serious disintegration of the joint;

they must threaten more speedy rupture into the cavity of the joint; and probably are less under the influence of genuflexion, which also must be attended with more risk of rupturing the sac. Aneurisms which grow forwards from the anterior or deep aspect of the artery are exposed to two grave complications—(1) they may grow into, erode, and irritate the bone (*vide* Tufnell's work), or (2) they may press upon the posterior ligament of the joint. In the former case they may burst after attaining a very small size; for in such cases it is clear that the pressure exercised by the tumour on the bone is reflected back upon its sac, causing it to soften and give way. In the second case, synovitis is apt to be an early complication, and the aneurism may suddenly burst into the cavity of the knee, as in Mr. Moore's case reported in the British Medical Journal, 1859.

3. Again, the form of the aneurism has much influence on its curability. Those tumours which communicate with the artery by a definite orifice, from which the sac stands out as a bud, are far more curable by gradual or partial compression; and in flexion it is quite possible that their orifice may be obstructed by displacing clot, leading to a rapid cure, though flexion may probably succeed by displacing the clot in any form of aneurism. The quantity of clot also in the sac when the patient is first seen, furnishes an important indication of the probable success of the milder methods of treatment.

4. Another practical consideration of the highest moment is the progress of the case while under observation, or in other words, the rate at which the symptoms are advancing. This rate depends in part, no doubt, on the particulars above alluded to—*i.e.*, on the composition of the sac, on the position of the tumour, and on the nature of its orifice; but also on many other circumstances of which no general statement is possible; such as the precise exciting cause of the aneurism, the minute anatomy of the affected artery, the patient's own habits or conduct, and all those individual peculiarities which are comprised under the vague term of his "constitution"—peculiarities which it is impossible to foresee, or even in most cases to explain, but which exercise so real and so vital an influence in the progress of aneurisms, not only when they occur in different persons, but also when they occur in the same person at different times. Anyhow it must be admitted that cases of popliteal aneurism display great variety in their progress: in some cases a few days' rest and quiet is all that has been necessary; in others, the surgeon must act at once and decisively, or serious if not fatal mischief will ensue.

5. The next important practical difference between different cases consists in the presence or absence of other diseases, of

which cardiac mischief, perceptible atheroma, or ossification of the affected or other arteries, and renal diseases, are those which first occur to the surgeon's mind.

Now, all these facts can usually be judged of in popliteal aneurism with some approach to certainty. When the artery crosses the back of the aneurism, it can be traced by a line of pulsation distinct from the general heaving of the aneurism, and particularly recognisable when blood is re-admitted after temporary compression of the femoral. When the tumour grows more or less directly backwards, its position may be inferred from the absence of the above symptoms, from the considerable œdema, and from the early existence of pain in the course of the nerve. The tumour may, however, lap round the artery, as in a case in St. Thomas's Hospital.

It is more difficult to judge of the condition of the orifice of the tumour. A very common state of parts in aneurism is that in which the tumour, originating from one definite portion of the vessel, which has given way from disease or injury, overlaps the artery for some distance, so that a certain portion of healthy artery above and below the orifice is enveloped in the sac of the aneurism. But it is equally common, taking all aneurisms together, to have an arrangement more resembling that of fusiform dilatation—there being two openings of the artery into and out of a more or less tubular sac. This arrangement, however, seems to Professor Holmes (judging from memory rather than from given data) to be less common than the others in popliteal aneurism. It is more common in the larger arteries than in smaller ones, and is less likely to occur in the ham, since aneurisms here seem so frequently to depend on partial rupture of the artery at a certain spot, while the tubular aneurism originates in degeneration of a considerable part of the vessel. The physical diagnosis must rest on the character of the bruit, on the shape of the tumour, and on the nature of its expansion. The phenomena observed when the tumour is as far as possible emptied, after compression of the artery above, and the blood then re-admitted, enable the surgeon to judge more or less certainly of the strength of the sac in various parts, of the amount of coagulum contained in it, and of the position of its orifice.

The first step, then, in the treatment of popliteal aneurism is to ascertain as accurately as may be these particulars with regard to the case in hand, and to base upon them the important choice of the method to be first adopted. The choice between the ligature and some form of pressure (including genuflexion amongst the latter) will rest chiefly upon the presence or absence of a thick, well-formed sac, lined with

some amount of coagulum; on the judgment of the surgeon as to the relations of the sac to its orifice; on the acute or chronic nature of the symptoms; and on the presence or absence of disease of the heart, arteries, or kidneys. The most promising cases for compression are those with a thick sac and a certain amount of fibrine deposited, without urgent symptoms or rapid advance, and where the characters of the bruit and of the pulsation point to the conclusion that the aneurism is not fusiform. And in these cases where the presence of other general complications render the surgeon anxious to avoid the risks attending a serious operation, compression has very often succeeded; but many cases of successful ligature of the femoral artery have occurred in patients suffering from cardiac or renal affections, or with aneurisms in other parts.

If compression does not succeed at first, how long should it be persevered with? This question can only be answered in very general terms. Considering the distress and disappointment, and sometimes acute suffering, to the patient of prolonged compression; considering, too, that experience up to the present time points to the conclusion that a patient is not more, but less likely to recover from ligature of the femoral if a prolonged application of pressure has previously been made; and if it can be shown that the Hunterian operation is much less dangerous than we have been taught to regard it, judging only from the experience of a former generation, recorded by Norris and other statistical writers,—it would be better to abandon the compression treatment after a careful, but very moderate, trial if it does not seem to be doing good, and then to resort to the ligature before the patient's chance of recovery from that operation has been permanently impaired. As to how long a time is implied by "a very moderate trial," that must be decided by the individual cases, but Mr. Holmes's own opinion is that more harm than good generally results from protracting the attempt beyond a week.

The results of each form of treatment as applied to popliteal aneurism are by no means easy to ascertain or to compare with each other. The comparison which is made between compression and the ligature is based upon statistics which, as applied to the latter, are composed mainly of cases operated on long ago, when the success of surgical operations was much less than it is at present. Reliable statistics of the compression treatment are very hard to procure; published cases afford no fair test of the average success of so common a proceeding, since they are recorded mostly on account of something unusual in their course. Hospital cases, if the records are complete, afford a better criterion of success obtained, but are open to the objection that better results might be obtained, if more minute atten-

tion were given to the cases than hospital arrangements provide. The records of hospital practice probably do not show all that might be done, therefore, by the method. Then, again, some of the most promising methods of compression are as yet new, while the rapid method under chloroform is almost untried in popliteal aneurism.

Again, we must remember, on the one hand, that compression is applied to milder cases than those which are subject to the ligature; and, on the other, that the treatment by compression is not yet perfected, but is still being developed and improved. Still, the experience of surgeons in the past will be found to give no uncertain indication of the direction in which improvements in the treatment of this disease are to be sought. The Hunterian operation was eagerly adopted in cases in which, up to the time of its introduction, no one had ever thought accessible to surgical operations at all. It was not merely successful in curing popliteal aneurism—which was at that time a most fatal disease, rarely cured by any means short of amputation,—but it also gave a new impetus to the study of the arteries and their diseases, and thus, by showing that some amount of circulation was not incompatible with the cure of aneurism, it laid the foundation for the introduction of that treatment by compression, which has since, but very wrongly, been sometimes looked upon as a rival or enemy of the ligature. Certainly John Hunter did not so regard it, for after having tied the femoral twice, he attempted the cure by compression in his third patient, and only tied the artery on the failure of that attempt.

Hunter himself only operated on five occasions; and although the operation was in its infancy, and as he at first took no care to exclude the vein, and tied the artery in a position where it is not very easily accessible, still only one case died. There can be no doubt that this fortunate result did much to insure the reception of the method into practice. Judging of the success of the operation from statistics published by Norris and by Porta, the mortality is about one-fourth; from statistics drawn from the London hospitals by Mr. Hutchinson, the mortality appears to be one-third; while the experience of individual operators who had enjoyed exceptional experience in the treatment of aneurism was said to have been far more favourable than this.

Professor Holmes has attempted to discover what the mortality of this operation has been in our hospitals (both metropolitan and others) of late years, and he finds it much less than is represented by the tables of Norris and Porta; and this he attributes to a concurrence of causes, mainly perhaps to the earlier treatment of cases by surgical operations since the intro-

duction of anæsthetics, but also to improved methods of operating, and improved knowledge of anatomy and surgery. This mortality is neither one-third nor one-fourth, but one-seventh, including several cases in which death occurred from causes quite unconnected with the operation. Nor are the London any worse than the country hospitals.

These remarks refer to cases of ligature of the femoral artery at once, uncomplicated by any previous pressure or other treatment.

In the table, exhibited by the Professor, of the results of treatment of aneurisms in our British hospitals, there are 212 cases of popliteal aneurism, and they furnish examples of every variety of treatment. In 77 cases the femoral was tied without any previous trial of compression, and of these 11 died.

In one of the fatal cases, however, the death was occasioned by small-pox. To these unfavourable cases those should be added in which the operation failed to cure the disease: these were only three, viz., one in which the old operation was performed subsequently with success, and two in which amputation was successfully performed. Thus, of the whole 77 operations, the failures were only 14, or at most 15, and out of these three were cured subsequently—so that the largest ratio of failures which can be ascribed to the Hunterian operation would be only 19·48 per cent.; and it should be recollected that this includes one-fifth of cases in which, though the operation itself failed, the patient survived, and was cured otherwise. These are the miscellaneous results of practice, on a collection of all the cases, published and unpublished, which occurred in the hospitals referred to, and the table represents the average results of practice as far as the numbers go; and these numbers are large enough to show the truth of the argument. In Norris's table, the operation proved fatal in 24·46 per cent. In Professor Holmes's table, gangrene occurs at most four times only out of 87 cases, and proved fatal in two or three; while in Norris's table (containing twice as many cases) it occurs 31 times, and proved fatal in at least 23 cases.

In the hospital table, again, there is an absence of any fatal case of suppuration of the sac; but in Norris's there are six deaths from this cause, besides ten cases in which recovery took place. Pretty nearly the same observations apply to secondary hemorrhage. It was the cause of death, directly or indirectly, in three of the fatal cases, and its occurrence is noted in one which recovered. In Norris's table it occurred in twelve fatal cases, and in the same number of cases which recovered. Hence it seems that ligature of the femoral has become much more successful than it used to be down to the time

of Norris's table, published in 1849; and as applied to the treatment of popliteal aneurism, it is really much more successful than the researches of previous writers have led us to believe, and perhaps has become more successful than it used to be.

In commencing the discussion as to the relative merits of compression, and the other bloodless methods of treating popliteal aneurism, it must be stated that we have in the Hunterian operation a method which almost always succeeds if the patient recovers from the operation, and which may be expected confidently to be followed by recovery in the very large majority of cases, especially if the dissection be skilfully and carefully conducted, so as to avoid all violence to the vein, and all unnecessary exposure or contusion of the artery.

The results of the treatment by compression, in all its forms, as shown by the hospital table, are as follows:—Compression was tried with success in sixty-six cases, and in fifty-eight without success. The most interesting questions in this record of experience are those which refer to the frequency of the failure of compression—to the effect which compression may have, if it fails, on the future progress of the case, and to the form of pressure which it is best to employ.

Professor Holmes addressed himself to these matters in their order:—

First, as to the frequency of failure. It seems discouraging, at first sight, that pressure should have failed fifty-eight out of 124 times. But these cases go back a long way in the history of compression, and many of them were under the care of surgeons who were by no means favourable to the use of pressure, and had no experience of the way of carrying it out. Then in hospital practice many little details, which in private receive the attention of the surgeon himself, are confided to the students and dressers, or to nurses. Besides, the great majority of these cases were treated by instrumental compression, and it is beyond all question that digital pressure is in every way superior.

Yet even if we allow that failure must continue to occur in half the cases so treated, we ought not to be discouraged from the use of pressure, however successful the ligature may be, unless there is some great reason for believing that it will spoil the prospects of the successful treatment of the case afterwards. For the application of pressure in favourable cases certainly exposes the patient to no risks, while the ligature of the femoral artery does. The only case in which death occurred while the patient was under treatment was one in which pleurisy accidentally supervened. But there can be no doubt that in this, as in other methods of treatment, disasters will occur. Thus,

there are eight cases in which amputation became necessary from gangrene, rupture, or suppuration of the sac; of these five died, and three recovered.

Nor must the fact be lost sight of that in some cases instrumental compression, and also digital pressure, when ill or carelessly applied, may produce gangrene by obstructing the vein, while either of these methods, if it does not cure, may easily aggravate the disease; and, finally, genuflexion has dangers of its own which lead to rupture of the sac, and sometimes to gangrene of the integuments which cover it. So that in a small proportion of cases, pressure has certainly spoilt the case so completely as to render amputation necessary. There are, too, cases in which pressure produces a direct effect on the artery, and some in which the tissues around the artery are much thickened and consolidated, so that the tissues above and around the vessel are brawny, the artery and vein closely connected together, and the walls of the artery thickened and hardened at the seat of pressure. All experience and authority lead to the conclusion that direct pressure on the structure of the vessels on which the force acts produces no visible or tangible anatomical lesions; but this position can hardly now be sustained, for every one is agreed that the action of long-continued compression is often plainly perceptible on the cellular tissue which supports and unites the vessels.

The results of compression are probably susceptible of much improvement, but nevertheless are yet satisfactory as far as they go.—*Med. Times and Gazette*, Aug. 8, 1874, p. 141.

53.—THE TREATMENT OF ANEURISM OF THE ARCH OF THE AORTA BY GALVANO-PUNCTURE.

By Dr. MCCALL ANDERSON, Professor of Medicine in Anderson's University, and Physician to the Royal Infirmary, Glasgow.

[The following, taken from a long article upon this subject, expresses Dr. McCall Anderson's experience of the best method of procedure in the treatment of aneurism by electrolysis.]

The kind of battery to be used is not of very much importance; the principal thing is to see that it is in good order. I prefer Stöhrer's battery, being accustomed to it, and perhaps it is better to use one the cells of which are large. As to the strength of the current, I am inclined to believe that, in some cases at all events, the want of success has been due in great measure to using it too strong, and that it is much better to employ a weak current, and, if necessary, to continue the operation for a longer period of time. The needles to be used should be sharp and not too coarse, and that portion which

penetrates the skin, the walls of the sac, and the intervening tissues should be insulated with vulcanite. They should be oiled before being introduced. In the majority of instances one needle is sufficient, but sometimes, especially in the case of large aneurisms, two or more may be necessary. There is much difference of opinion as to how the needles should be connected with the battery. I prefer connecting them with the positive pole. In this respect I differ from Dr. Althaus, who says: "From the results of the foregoing cases, I have no doubt whatever that the most effective application of the current is that when both poles are inserted into the sac. This mode of application is also that employed by Ciniselli and Dr. Duncan of Edinburgh. Both poles are useful in different ways. The positive produces a small firm clot, and the negative a large soft one. When only one pole is in the sac the resistance encountered by the electricity is so great that a much larger galvanic power has to be used to produce any effect at all, and even then the effect of that pole which remains outside is lost." And yet it is a very curious circumstance that one of the most successful cases recorded in Dr. Althaus' book is the one which I have last detailed to you, in which the needle was connected with the positive pole alone, and a very weak current was employed. I prefer connecting the battery with the positive pole because the clot which forms at the positive is much firmer than that which forms at the negative, and because there is less danger of hemorrhage. This is well illustrated in the first case which I brought under your notice to-day, and also in several of the cases related in Dr. Althaus' book. The hemorrhage is objectionable because, besides being an unpleasant complication, it alarms the patient and is calculated to excite the circulation, and because it is a proof that the coagulation at the negative pole is of a very unsatisfactory character. To sum up, then, although further experience on my own part and upon that of others may tend to modify my opinions on the subject, I am in favour of using a battery with large cells, of employing a weak current, of using one or more needles, insulated with vulcanite at that part which penetrates the skin and walls of the sac, and of connecting these needles in every case with the positive pole of the battery.—*Lancet*, June 20, 1874, p. 864.

54.—CASES OF HEMORRHAGE FROM THE PALM OF THE HAND, TREATED BY PRESSURE.

By E. L. HUSSEY, Esq., Senior Surgeon to the Radcliffe Infirmary, Oxford.

Case 1.—A farm labourer, forty-eight years of age, living at Dorchester, about nine miles from Oxford, was admitted into

the Radcliffe Infirmary under my care on September 11, 1868, on account of sudden and profuse loss of blood from a swelling in the palm of the left hand.

History.—The man is left handed. About seven years ago, after some hard work, he found a swelling in the palm of his left hand. As it continued to increase in size, he showed it at different times to Mr. Byass, of Dorchester, and his assistants. They informed him of the serious nature of the case, and of the necessity for treatment, telling him that he might some day bleed to death from it. Mr. Byass mentioned to me in conversation that he had such a case under observation. About twelve months ago the swelling was punctured by a practitioner in the neighbourhood; some slight flow of blood followed the puncture. The wound healed favourably, but the swelling grew more rapidly in size afterwards. The patient described it as becoming raised, and even pendulous; the apex of the swelling became softer. His fingers became permanently flexed, and he had a difficulty in opening the hand enough to hold his tools for work in the fields.

On the morning of admission, after fomenting the hand with flannel and warm water, he tried to straighten the fingers forcibly. The swelling burst, and before he could obtain help, he lost, by his own estimate, about three quarts of bright florid blood. The friends about him tied a cord round his arm, and sent for Mr. Byass. Mr. Harris, Mr. Byass's assistant, who was well acquainted with the nature of the case, attended at once. A thick compress of lint was placed in the palm, and a bandage applied round the hand and wrist, and a tourniquet buckled on the arm above the elbow, with the pad over the brachial artery. The patient was then sent in a cart to the Infirmary. Upon admission he was sent to bed, with his hand and arm raised on a pillow.

The next day I saw him. Upon examining the patient, and hearing the history, I formed the opinion that the swelling was an aneurism of the palmar arch, and I determined to try the effect of pressure. The bandage, which had become hard and dry, was cut away, leaving the compress adherent to the palm. The thumb and fingers were bandaged separately with narrow strips of calico, carried evenly from their extremities upwards toward the hand. These were fixed, with the fingers flexed upon the hand, and firm pressure made by carrying several turns of a calico roller over the compress and round the hand. Compresses of lint, about four inches in length, were laid along the line of the radial and ulnar arteries, and secured by a roller bound firmly round the wrist and forearm, as far as the bend of the elbow. The hand was placed across the chest, and the limb confined in a sling, the forearm being flexed somewhat

beyond a right angle. The man was ordered to remain in bed, and directions were given to the nurse that he was not to be raised from the horizontal position, and that the arm was not to be moved, nor the forearm straightened for any purpose.

On the 17th some slight amount of blood escaped from under the dressings, but not enough to make me think it necessary to remove them for the purpose of examining the wound.

The dressings were all removed on the 21st. A compress of lint was again laid on the opening in the palm, and fresh bandages applied to the fingers, hand, and forearm, making firm and uniform pressure as before. These were allowed to remain for about ten days. The discharge became purulent, and was for some time mixed with clots of putrid blood. The bandages were all removed, and a piece of linen rag with simple cerate was left on the wound. The pressure was discontinued, and the man was allowed to move about the ward with his arm in a sling. By degrees the suppuration ceased, and the man left the Infirmary on October 28, with the wound nearly healed.

On July 20, 1872, he came to the Infirmary, and I saw him. A very small scar, not so large as a split pea, can be seen in the palm of the hand. The skin moves easily over the palmar fascia, and the tendons play freely. The power of flexion and extension is perfect. The man says the arm is stronger than the other. Both arteries can be felt at the wrist, but the radial is fuller than the ulnar.

Case 2.—A shoemaker, fifty years of age, was admitted into the Radcliffe Infirmary under my care on February 27, 1872, having lost a large quantity of blood from a deep wound recently inflicted in the palm of his left hand. I was in the ward at the time, and saw the man immediately on admission.

The wound was in the middle of the palm, about half an inch in length, in the line of the metacarpal bones, passing obliquely through the skin and palmar fascia. It was caused by his accidentally sticking his knife into it when seated at his work. A profuse loss of blood followed immediately—to the amount, he said, of two quarts. A fellow-workman tied some handkerchiefs round the hand, and the man came to the Infirmary, a distance of about two miles.

Having laid the man down in the horizontal position, the cloths, which were saturated with blood, were removed, so that I could examine the wound. I had no doubt that the injury was a puncture or division of the palmar arch, and I determined to make use of pressure, which had been so successful in the former case. A compress of lint was placed on the wound in the palm; narrow bandages were wound round the thumb and

fingers separately, and also round the hand. Compresses were laid along the line of the radial and ulnar arteries, and a bandage carried round the wrist and forearm up to the bend of the elbow; firm and uniform pressure being made over the fingers, hand, and forearm. The forearm was flexed upon the arm to rather more than a right angle, and confined in a sling across the chest. The patient was placed in bed, with directions that he should not be raised nor the arm moved from the position in which it was placed.

On March 4, the bandages having become slack were removed, and the compress taken from the wound without any loss of blood. A fresh compress of lint was placed upon the wound in the palm, and others along the radial and ulnar arteries, and the bandages reapplied, in the same manner as before, over the fingers, hand, and forearm. The forearm, flexed to a right angle, replaced in a sling, and the same directions continued for keeping the arm at rest. The wound healed without suppuration. The man left the Infirmary on March 13, and soon afterwards returned to his regular work as a shoemaker.

In June, 1874, Dr. Spencer, the medical officer of the sick club of which the patient is a member, saw him. He tells me that both arteries can be felt at the wrist; the ulnar, he thinks, beats more forcibly than the radial. The man has continued at his work with the free use of his hand.

Remarks.—In the first of these cases, in the absence of accurate knowledge of the exact nature of the aneurismal swelling and its anatomical relations, an attempt to reach the source of hemorrhage by operation in the palm was not one likely to be attended with success. In the other case, where the patient came under notice at an earlier period after the injury, a less unfavourable case presented itself for an attempt to tie the wounded vessel at the seat of injury.

It cannot be said that wounds of the palmar arch form an exception to the known rule that in a case of hemorrhage from an artery of sufficient size to require a ligature, both ends of the divided vessel should, in all cases where possible, be tied at the seat of injury. But it is an almost insuperable objection to an operation for such a purpose, even in a healthy condition of the palm, that it involves a tedious and difficult dissection among parts which cannot be divided or separated without risk of permanent injury.

The cases which have been reported show that stopping the current of blood by ligature of the trunks supplying the palmar arches—whether the radial and ulnar at the wrist, or the brachial at a greater distance—has not secured the patient from a return of hemorrhage.

These two cases are offered to the readers as instances of the success of pressure carefully and efficiently applied. The object of the pressure is to check the force of the whole circulation in the limb; not only the current through the larger arterial trunks—the radial, ulnar, and interosseous, in the forearm and at the wrist—but also through the smaller vessels and capillaries in the hand and fingers, until the injury is repaired. This is not done effectually unless the pressure is made equally on the distal side of the palmar arches, beyond the aneurism, or other seat of injury.—*Med. Times and Gazette*, Aug. 29, 1874, p. 231.

55.—THE TREATMENT OF PALMAR ANEURISM.

By WILLIAM MAC CORMAC, Esq., M.A., Assistant Surgeon to St. Thomas's Hospital.

[This paper was elicited by one on the same subject by Mr. Cripps, who related a case of traumatic palmar aneurism, which illustrates the serious nature of the disease when treated on the established principle of laying open the sac in order to secure the wounded points.]

Five cases are mentioned by Mr. Cripps as having been treated in this way; three died of secondary hemorrhage, and two were cured, one of these being Mr. Cripps's own case, in which, after several recurrences of bleeding, the ulnar radial arteries were first tied, but without avail, then the brachial with success. Nine cases were cured by pressure, two others were treated by ligature, and one by injection. I can add another case to the list cured by pressure, which will, perhaps, be of interest in connection with the preceding one. On May 1st, a healthy-looking man, aged 30, was admitted to St. Thomas's Hospital under my care. He had cut the palm of his hand a month previously with a saddler's knife, near the ulnar border. The wound, which was strapped up by a doctor, soon healed, and for three weeks he experienced no trouble. Then the hand began to swell, and he experienced much pain. On admission, a week after, the tumour was of the size of a walnut, occupying the ulnar border of the palm close below the wrist. The skin over the most prominent part was thin and red in colour; the pulsation was very strong, and the tumour very tense. Compression of the arteries at the wrist stopped the pulsation, and the sac could be partially emptied. Subcutaneous injections of ergotin, with instrumental pressure on both radial and ulnar arteries near the wrist, were tried until the following day. No good effect followed; the tumour was larger, more tense, and the skin seemed likely to give way. The forearm was now carefully bandaged with flannel, placed

upon a posterior splint, and well elevated upon pillows; the tumour itself was left uncovered, and an ice-bag laid upon it. Digital pressure was now applied to the brachial artery, the directions given being, that no drop of blood should be allowed to pass. After an interval of forty hours, the aneurism was found to have become harder, and without a trace of pulsation. The following day, it was imagined that slight pulsation could be detected; this became next morning somewhat more distinct, and was perceptible during the next eleven days; but it was always very faint, and the tumour, nevertheless, continued during the whole time to diminish steadily in size. By way of precaution, digital pressure was applied afresh for twenty-four hours without altering the condition of matters, and the arm was constantly kept upon the splint. On the 16th, a fortnight after the pressure was commenced, it was observed that, during three or four hours at a time, the pulsation wholly disappeared; would then return for a few hours, and again disappear. During this interval until the final disappearance on the evening of the 16th, it was clear that the cause of the pulsation was a supply of blood from the radial artery; pressure on that artery stopped the pulsation; pressure on the ulnar artery produced no effect whatever.

To-day, May 26th, I may pronounce the patient cured. There has been no pulsation in the aneurism during the last ten days. The tumour is being gradually absorbed, and is now about one-third its original bulk. I conclude that the return of pulsation was due to the contraction of the soft clot formed in the sac, allowing a certain quantity of blood to enter from the distal or radial side. So far as it goes, this case encourages me to further employ pressure in these cases, and I feel convinced that it is better, and more likely to prove successful if we at once apply the pressure to the main trunk, rather than to the radial and ulnar arteries. The long and superficial course of the artery renders it very easy to compress without inconvenience or suffering to the patient. Where the alternative lies between tying the brachial artery, the radial, and the ulnar, I am inclined to think the former the preferable course, as affording greater security against recurrent hemorrhage.—*British Medical Journal*, May 30, 1874, p. 707.

56.—CASES ILLUSTRATING THE USE OF THE ELASTIC LIGATURE.

Under the care of LAWSON TAIT, Esq., at the Birmingham Hospital for Women.

The value of the elastic ligature is well illustrated in the subjoined cases. In the first case it was specially suitable. It

is, indeed, in the instances of vascular growths so situated that cutting operations are inadmissible, that the gradual strangulation chiefly triumphs. The *modus operandi* tends materially to diminish the risk of hemorrhage, while in operations of no great magnitude the danger of the supervention of phlebitis is but small, at least not greater than after the use of the knife, the cautery, injections, or the ordinary ligatures.

M. B., aged twenty-five, admitted Dec. 25th, with a large varicocele of left labium, which gave rise to much discomfort from the excoriations and discharge. Its removal had been attempted in another institution, but the hemorrhage had been so alarming that it was not persisted in. According to her own account, the patient had to be watched night and day by dressers.

Feb. 2nd. Mr. Tait passed a double elastic ligature through the base of the tumour by means of a trocar, and tied it in opposite directions, so that each half of the ligature embraced half of the base of the tumour. A quarter of a grain of morphia was at the same time injected under the skin of the arm.

4th. The tumour quite black, and nearly separated.

7th. The separation is complete, and a healthy granulating surface about three inches in diameter is left, to which red lotion was applied as a dressing.

Very little pain was complained of after the first twenty-four hours, and there was never the least hemorrhage. On the 23rd of the month the wound was almost healed.

H. S., aged thirty-three, admitted Jan. 5th with a deep and very sinuous fistula leading from about two inches to the left of the anus through the ischio-rectal fossa to an aperture in the rectum about three inches up. An elastic ligature was passed and tied on the 26th, and it came out on the 30th. Considerable pain was felt for a few hours after its insertion. The track healed perfectly, and the patient was discharged cured. In this case the advantage of the ligature over the knife was that it saved all loss of blood, and, as the patient was very anæmic, that was a point of importance.

M. P., admitted April 27th, suffering from three perineal fistulæ, one opening to the right of the right labium, and the others about an inch and two inches respectively to the right of the anus. The uterus was quite fixed, and the history given indicated the occurrence of a pelvic hæmatocele some months previously, and its subsequent suppuration. These fistulous tracks led up into a cavity behind the uterus, from which a very abundant discharge flowed after its exploration.

May 1st. Two elastic ligatures were passed, one through the track opening in the labium, and the other through the prin-

cipal track to the right of the anus into the suppurating cavity, and thence through an opening made into the rectum, and then they were tied through the rectum. They made their way out on May 4th and 5th. A few days later the discharge was coming entirely through the rectum.

19th. The discharge has very much diminished in quantity, and the patient is now able to sit comfortably, as she has not done for many months.—*Lancet*, June 27, 1874, p. 905.

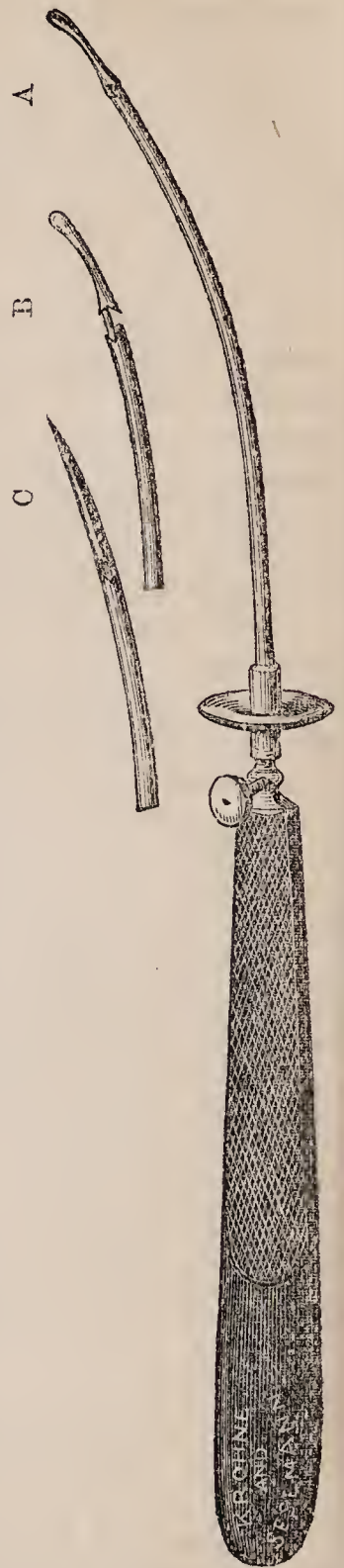
57.—A SUBSTITUTE FOR THE ELASTIC LIGATURE.

At a late meeting of the Clinical Society, Mr. Callender exhibited for Dr. Hollis "A Sarcotome." This instrument consists essentially in substituting, as a cutting apparatus, a spiral steel spring and a waxed thread for the ordinary caoutchouc tubing, which has hitherto acted as an elastic ligature. The spring is confined in a small metal tube, A, closed at one end and wormed externally as a screw for about an inch at the other, in order to receive a ring of metal called the "screw ring." A short cylinder, B, slides readily over A, and is fixed to one extremity of the spring by means of a screw which passes through two longitudinal slits in the sides of A. A metal cap having a small ring at its free end fits on the closed end of A. When arranged for use, the free end of the spring is forced against the closed end of A by the "screw-ring" and sliding tube, B. A waxed thread or other ligature is placed around the parts to be screwed; the surgeon passes its end through the ring in the cap, and then fastens them by means of a screw nut fixed to B. The string is released by removing the screw ring, and its whole pressure is directly exerted upon the ligature. Dr. Hollis claims for this invention many advantages over the elastic ligature. It can be used with any form of thread or wire ligature, thus diminishing the risk of breakage; it is easily adjusted to a constant known pressure; and, in as far as it has already been applied by Mr. Callender, it appears absolutely painless in its operation.—*Medical Times and Gazette*, May 23, 1874, p. 568.

58.—MR. ALLINGHAM'S INSTRUMENT FOR FACILITATING THE INTRODUCTION OF THE ELASTIC LIGATURE.

There can be little doubt that many simple surgical operations may be advantageously performed by means of the elastic ligature; but the difficulty experienced in getting the india-rubber cord through a fistula in ano, or several times under the base of a tumour, so that it may be cut through in segments, has materially detracted from the general usefulness of Professor Dittel's method.

Mr. Allingham has devised, and Messrs. Krohne & Sesemann have perfected for him, a very simple instrument for drawing the rubber through a fistulous passage or beneath a tumour. It consists in the combination of a concealed hook or notch with a blunt or sharp pointed probe, as the case may require. A shows the curved probe with the hook concealed by the sliding canula, ready to be passed through a fistula, or, if a sharp point be substituted for a blunt point, under a tumour. B exhibits the instrument with the canula drawn back and the previously concealed notch exposed, ready to receive the loop of india-rubber; when this is placed in the notch the canula is pushed home and the ligature is held so firmly that it cannot escape. Thus a double ligature can be readily drawn through a fistula or under a tumour. It is not necessary, in fistula, to see the hook, for if the finger with a loop of india-rubber around it be passed up the rectum, the loop can with great facility be directed over the end of the probe and caught in the notch quite unassisted by vision. C shows the sharp-pointed instrument adapted to the same canula, so that only one handle and one canula are required to complete the double instrument.—*Lancet*, Aug. 1, p. 162.



59--MINOR SURGERY CLAMP.

Mr. Richard Davy, surgeon to the Westminster Hospital, has invented a very handy clamp for the elastic ligature, made by Blaise and Co., 69, St. James's-street, S.W.); and it has been used successfully for operations on the phalanges, circumcision, removal of warts from penis, &c. The same mechanism may be readily adapted for major operations. The ends of an elastic band are placed at one aperture of a tunnelled handle; the loop occupying the other. The surgeon, having made sufficient traction on the elastic loop around the phalanx or penis, turns

the screw on, and fixes the bands. The assistant's hold on the handle is very conveniently out of the way of the operator.—*Lancet*, August 1, 1874, p. 162.

60.—THE ELASTIC LIGATURE AND THE ELASTIC BANDAGE.

By S. MESSENGER BRADLEY, Esq., Manchester.

I lately had a case which illustrated very well the advantages derivable from the elastic ligature. A patient suffering from fistula *in ano*, complicated with stricture of the rectum about four inches from the anus, had the fistula divided in the usual way with the knife, when it was discovered that on one side the large abscess which had formed outside the bowel communicated by a long and sinuous track, with the bowel above the stricture. It would have been hazardous to lay open this track with the knife, as it entered the rectum at the point where the right fork of the superior hemorrhoidal artery divaricates from the left. A ligature, was, therefore, passed from the ischial opening of the abscess along the fistula, and brought out through the anus. In two days, this ligature had cut its way sufficiently to allow the wire-ecraseur to be applied, with which the operation was completed speedily and bloodlessly. While this case is a good example of the advantages, another which was under my care about the same time illustrates the disadvantages of this mode of treatment. A healthy boy of 14 presented himself among the out-patients of the Manchester Royal Infirmary, with a sessile fibrous tumour of the size of a large walnut, situated on the back of the metacarpal bone of the thumb. An elastic ligature was applied around the base, a small sulcus being first made with the knife, in which it was looped. Great pain was experienced for the first five or six hours, which, however, gradually subsided, and in seven days the ligature, tightened from time to time, had completed its work, and cut through the base of the tumour. Instead, however, of the advancing granulations, so pleasantly described by Professor Dittel as following the contracting ligature, an angry edge of acutely sensitive and inflamed tissue was left, whence erysipelas spread up the arm, and for a few days the boy was in decided peril. Two months passed before the cicatrix was complete. It seems probable, that the same modified praise is to be given to Esmarch's elastic bandage; for, while nothing but good is likely to ensue from its application in such cases as the one which Esmarch so graphically described, viz., the removal of carious bone in an anæmic patient, it cannot but fall into desuetude if it be indiscriminately employed

in all cases, particularly in those where some septic condition is present, or when amputation is practised on account of the presence of some diseased growth. In such cases, not only is it undesirable to squeeze back the germ-laden blood into the general circulation, but, the blood in the limb being already lost to the heart, it is unwise to burden a perhaps already weakened organ with propelling more blood than the system requires. The tourniquet saves to the system all that the system can benefit by, and to return more than this, except in rare cases where we contemplate transfusion, is unnecessary, if not actually harmful. Even at the hip, the abdominal tourniquet is sufficient to restrain undesirable hemorrhage, and in fat subjects, when it cannot be easily applied, it is probable that a plan suggested to me by Mr. J. Beswick Perrin, of transfixing the limb just below Poupart's ligament with a long pin from fourteen to eighteen inches in length, and then compressing the femoral artery by means of a twisted cord between this pin and the integument, would answer the purpose better than the elastic ligature. It is certain that it is easy, in the dead subject, by this means to perfectly compress the vessel and yet leave room for the passage of the knife.—*British Medical Journal*, May 2, 1874, p. 577.

61.—HYPODERMIC INJECTION OF ERGOTIN IN PURPURA HEMORRHAGICA.

By Dr. WM. L. LANE, Dunfermline.

The following are the brief notes of a new and successful method of treating purpura hemorrhagica. This case was, however, a complication of typhoid fever, consequently a case of even graver prognosis than an uncomplicated one of purpura hemorrhagica. Miss M., aged 16, was attacked with typhoid fever, under which she was progressing favourably, when, on February 16th, three weeks from the commencement of the fever, the patient had a severe attack of epistaxis, which recurred each successive time with greater severity. Forty-eight hours from the first attack of bleeding from the nose, several purpuric spots appeared all over the body simultaneously, and hæmoptysis, hæmatemesis, and hæmaturia set in. The patient took every kind of styptic, notwithstanding the the hemorrhages increased most alarmingly, so much so that she had several attacks of syncope. Seeing her case now quite hopeless, and finding that I had ineffectually used every kind of astringent recommended by various authorities for purpura hemorrhagica, such as turpentine, gallic acid, aromatic sulphuric acid, &c., I resolved, as a last resource, to give hypodermic

injection of ergotin a trial. I had previously found it very useful in hæmoptysis and hæmatemesis. Having also considered purpura hemorrhagica a disease of the capillaries, and not a malady the result of a deficient amount of fibrine in the blood, for the blood of purpuric patients coagulates as firmly as that of healthy individuals, and knowing that ergot always causes contraction of involuntary muscular fibres, I hoped that in this case, by its contracting the involuntary muscular fibres of the minute arteries, I should prevent any more hemorrhage by lessening the flow of blood to the capillaries. I injected hypodermically one grain of the liquid extract of ergot at a time. After the first injection, the hemorrhage from the nose, the stomach, the bowels, and the bladder diminished greatly, while fewer new purpuric spots appeared on the body. The second injection completely arrested the hemorrhage. The patient being nourished and stimulated freely her strength rallied, the fever came to a crisis a few days afterwards, and she made a perfect recovery.

This case is of interest in this respect, that it is the first time, as far as I am aware, that ergot has been used hypodermically for purpura hemorrhagica, and its employment has been followed by success.—*British Medical Journal*, Sept. 5, 1874, p. 304.

ORGANS OF RESPIRATION.

62.—ON LARYNGOSCOPY.

By Dr. GEORGE JOHNSON, F.R.S., Professor of Medicine in King's College; Physician to King's College Hospital.

One of the most useful means of acquiring skill and confidence in the examination of the larynx, is the practice of auto-laryngoscopy, that is, the examination of one's own larynx. The simplest and, on the whole, the most satisfactory method of auto-laryngoscopy was devised by myself eleven years ago. (See *Medical Times and Gazette*, Feb. 14, 1863, p. 157.) This method of auto-laryngoscopy requires no special apparatus. Sitting at a table of convenient height, I place a common looking-glass in front of me, and a moderator or gas lamp on one side of the glass, but two or three inches further back, so that the light may not pass directly from the lamp on to the face of the glass. Now, with the reflector on my forehead, I direct the light from the lamp, as it were, into the open mouth of my own image in the looking-glass; then introducing the laryngeal mirror into my mouth, I see the reflection of my larynx in the glass before me, and any one looking over my

shoulder can also see the reflected image. If I wish to demonstrate my larynx to several persons at once, I do this by having the mirror in front of me of small size—about three inches by two inches (fig. 1). Thus, while three or four per-



Fig. 1.—Auto-laryngoscopy and Demonstration. *a.* The small mirror on a brass stem.

sons standing behind me can see the reflection of my larynx in the mirror in front, three or four others, standing before me and looking over or under or by the sides of the demonstrating-glass, can see the reflection of my larynx from the laryngeal mirror which I hold in my mouth. Those who stand in front of me have a somewhat better view than those who look over my shoulder from behind, for the obvious reason that some loss of light attends the second reflection from the demonstrating-mirror before the image reaches the eye.

For beginners in the art of laryngoscopy, this method affords a very useful means of training and practice. One of the chief difficulties at first is to keep a steady light in the patient's mouth while the laryngeal mirror is being introduced. Now the student, after arranging his looking-glass and his lamp, may direct the light into his own open mouth in the looking-glass. This process differs scarcely at all from that which he will have to practise on his patients. Then having learnt to keep the light steadily and automatically directed into the mouth, he may warm and introduce the laryngeal mirror, and he will soon see

his own larynx. The frontal reflector is a very useful means of lighting the throat for the purpose of examining the tonsils, palate and pharynx. Placing a lamp or candle by the side of the patient, the operator, with the reflector on his forehead, throws the light into the throat, and has both his hands free to depress the tongue and to apply caustic or other local remedies. In cases of diphtheria and scarlet fever, by this method of illumination a thorough examination of the throat can be made in a much shorter time than by the ordinary method, and without raising the patient's head from the pillow. The operator, too, in this way, runs less risk of infection from inhaling the patient's breath, or from the morbid secretions being coughed into his face.

I had practised laryngoscopy for some years before I discovered that the slight nervousness experienced by most patients on having their throats inspected for the first time causes them to hold their breath; and this suspension of breathing soon distresses the patient and embarrasses the operator. The direction which I now give to my patients is in substance this: "While I introduce the mirror and look at your throat, do not hold your breath, but continue to breathe in and out freely;" and I do not attempt to introduce the mirror until I see that the patient understands and obeys my direction. An analogous instance of nervous apnoea sometimes occurs when a timid patient is about to inhale chloroform. Emotional excitement may so completely suspend the respiratory movements, that the patient has to be encouraged and instructed to continue his breathing before the inhalation can proceed. I was told lately by a photographer, that a nervous subject, when told to remain still, will sometimes, as it were, forget to breathe. The result is, that the sitter soon feels distressed, and has a pained expression of countenance. A medical friend once, when examining with me the photograph of a distinguished surgeon, said, "He looks as if he were passing a renal calculus." He had probably held his breath while sitting for his photograph.—*British Medical Journal*, May 23, 1874, p. 677.

ALIMENTARY CANAL.

63.—DESCRIPTION OF A NEEDLE GUARD, FOR USE IN OPERATIONS FOR HARE-LIP AND OTHER PLASTIC OPERATIONS ON THE LIPS AND FACE.

By HENRY J. TYRRELL, Esq., Surgeon to the Mater Misericordiæ Hospital, Dublin.

In operations for hare-lip and in other plastic operations on

the lips and cheeks, how to deal with needle points has been to me for a long time a source of difficulty.

Formerly I was in the habit of breaking them off, but I was not pleased with this mode of procedure, for the following reasons:—1. If the needles are tolerably strong (which they should be) the force necessary to break them causes a shock quite sufficient to disarrange the neatly and accurately adjusted edges of the wound. 2. At the place where the needles are broken, or cut, a rough edge is left, which renders their withdrawal more difficult and painful than if they had not been broken or cut. 3. When withdrawing a needle, it is much more easy to do so (and consequently, the edges of the wound are less likely to be torn asunder) if it is sufficiently long to be grasped, and rotated between the finger and thumb before traction is used.

For these reasons for some years I have been content to stick pieces of cork on the needle points, and although it looks a clumsy proceeding, I much preferred it to the more usual practice.

I have lately devised a simple contrivance, which I think of service in such cases. I call it a needle guard. It consists of two little plated tubes, each three-quarters of an inch long, and open at one end. The tubes are tied together with a piece of string elastic, or better still, with a vulcanised india-rubber ring, sufficiently extensible to allow the guard to be easily slipped over the two ends of the needle.

The guard does not interfere with the twisted suture or skin and can be at once removed, without the slightest difficulty or dragging, when the needle is about being withdrawn.

If the surgeon is “of a frugal mind,” and wishes to use the same needles again and again, he can do so by simply running them, after use, to and fro for a moment through an emery bag. The friction restores their polish and sharpness.

The guard was made for me by Messrs. Read, Parliament Street.—*Dublin Journal of Medical Science*, Sept. 1874, p. 223.

64.—ON THE REMOVAL OF HEMORRHOIDAL AND OTHER TUMOURS.

By HENRY LEE, Esq., Surgeon to St. George's Hospital.

Dupuytren and Cline were in the habit of removing internal piles by cutting them off with a pair of scissors. Sir B. Brodie at one time adopted this practice. In about two-fifths of Dupuytren's cases subsequent hemorrhage occurred, and sometimes to a very considerable extent. Sir B. Brodie, during the short time he tried the practice, states that, in the first one or two cases, he found no inconvenience to arise from his altered

practice; but then a case occurred in which the patient lost a great deal of blood; in another case the hemorrhage was so great that the patient nearly died; and then a third case occurred in which also the patient lost an enormous quantity of blood. After that time Sir B. Brodie always removed large internal piles by ligature. The following is the plan he recommended:—If the piles be of large size a large curved needle is passed through the base of each, armed with a strong double ligature. The double ligature is divided into two single ligatures, “which are tied round the base of the pile, one on one side and the other on the other side, with a single knot. . . . You then proceed to another step of the operation: cut off the convex portion of each pile, so as to make an opening into the cavity of the convoluted vein which forms it. Thus you take off the tension produced in the pile by the blood which it contains, and are enabled to draw the ligature tighter than before. . . . You have now only to complete the double knot upon each of the ligatures, and cut off the threads close to the knots, returning the piles, ligature, and all into the rectum.”

Now in this mode of performing the operation there is one source of danger which has been overlooked. It may so happen that a large vein is transfixed by the needle, and its two sides held separate. Its open mouth would then be bathed in the puriform secretions of the parts, and the conditions would be those most favourable for the absorption of those secretions. I have examined the body in more than one case where death might have been attributed to this cause. In one instance in particular, where the operation was performed by a most experienced surgeon, secondary deposits were found in the liver and in no other part—showing that the absorption must have taken place through the superior hemorrhoidal veins. This particular cause of danger may be avoided by carrying the ligature from one side, after the double knot is tied, round the whole tumour, as has often been done, and tying it on the opposite side.

Another objection to this mode of operating is the pain. In order to draw the ligatures very tight, the parts must be pulled down to a level with the hands. This causes very considerable suffering; and although, for the moment, or under the influence of chloroform, this may be of no great consequence, yet there can be no doubt that the less the parts are disturbed the less risk there will be of subsequent inflammation. I have known a number of patients who have complained most bitterly of the pain that has been caused by the operation of tying their piles, and this not only at the time or immediately after the operation, but sometimes for several days. This pain I have attributed in great measure to the forcible dragging down of

the parts below the level to which they would naturally be protruded.

A third objection to the operation by ligature is the comparative length of time that the patient is kept in bed. The ligature has to eat its way through, and some portion of the pile necessarily sloughs off before the process of repair can commence; a week may thus be lost, during which time generally there is more or less constitutional disturbance.

The old operation which I have now described was, on the whole, a good one and generally successful, but patients who have undergone it have often warned others against undergoing the same. Some patients have an instinctive dread of having any cutting or of having needles used. The operation may then be performed with safety and without dragging upon the parts by the elastic ligature. I have performed several operations in this way. The pile is seized with a pair of forceps which closes with a spring or a screw and terminates in a ring. The elastic ligature is carried with the point of the finger several times round the end of the forceps and tied. The protruding pile or piles are then returned into the rectum. They slough off in the same manner as if tied with a silk ligature, but there is this additional security—namely, that the ligature always remains tight. If the pile be large, and a ligature is applied, after it has eaten its way partly through it becomes loose, and should the pile not be completely dead the circulation in it may be in part restored, and there is some danger of the products of the decomposing mass becoming absorbed.

It is astonishing with what rapidity the elastic ligature will cut its way through. Not long ago a case of fistula in ano was under my care in St. George's Hospital. Not wishing, for some cause or other, to perform the usual operation, I passed several threads of elastic ligature through the fistula, and brought them out of the rectum. They were then tied moderately tight. At the expiration of forty-eight hours I was going to farther tighten the ligatures should it be found necessary, when, to my surprise, they came away in my hand, having completely divided all the structures between the fistula and the bowel. The elastic ligature, if used for this or for other purposes, should be solid. If tubular, when stretched it flattens against the parts, and, consequently, does not cut through them so easily.

The objections mentioned with regard to Dupuytren's and Brodie's modes of removing piles apply in some degree to a modified operation which has been extensively practised of late years. It consists in a combination of cutting and tying. The tumour to be removed is drawn down by a hook armed with several sharp teeth. The mucous membrane is cut through at

its sides and lower parts with a pair of scissors. The remainder of the pile, which generally contains its arterial supply, is then tied. The portion of the tumour which projects beyond the ligature is cut off, and the remainder is returned into the rectum.

By far the best and safest way, according to my experience, of removing a pile, is to grasp it at its base with a clamp made in the shape of a pair of scissors, curved on the flat, and with the points turned inward; then to cut the pile off with a pair of scissors of the same shape, and to sear the cut surface with an iron after it has passed from a dull-red to a black heat.

The advantages of the clamp are: 1. That, being curved horizontally, it can be inserted between the nates, so as to grasp the base of the pile, without the latter being forcibly dragged down. 2. From the limbs of the instrument being curved inward, every portion of the pile is equally compressed, and no part can possibly slip. 3. One blade is made to play within the other, so that they cannot be displaced by any horizontal pressure. 4. The blades are sufficiently thin to allow nearly the whole even of a small tumour to project, and consequently to be removed, on the concave surface of the clamp.

Operating with this instrument, I have hitherto had no case in which there was any hemorrhage to signify. About a year ago I removed a large portion of the tongue with a similar instrument; and about a month from the time I am writing I removed a testicle affected with soft cancer, which weighed something less than a pound. The cord was seized with the clamp and cut with a pair of curved scissors. The cut surface was then seared in the way described. No ligatures were required. The wound was closed with carbolised sutures, and healed very satisfactorily, although the patient had an attack of erysipelas of the skin covering some enlarged glands in the opposite groin.

The first curved screw clamp which I had made was in 1839. It consisted of two parallel metal plates curved on the flat, with a screw at each end. This instrument I have still in my possession, and have lately used it in removing a very large mass of epithelial cancer from the scrotum. Various modifications of this instrument, and the mode of using it, have at different times been adopted. Mr. Matthews, of Portugal Street, many years ago made for me a clamp in the shape of a pair of scissors, with the blades curved in both directions, as above described. Mr. H. Smith adapted a screw to the handles of a similar-shaped instrument, and this was a great improvement; but the instrument which at present passes under Mr. Smith's name has straight, thick, parallel blades, which render it difficult to be used when it has to be introduced.

between projecting parts. When the tumours are small, they are often compressed between the blades, instead of being protruded beyond them. If the blades of an instrument are parallel, the pressure which it exerts must be chiefly on the centre or thickest part of the tumour. The parts of the tumour near the point and the handle of the instrument, unless very great force is used, have comparatively little pressure exercised upon them; and these parts, as is found in practice, are very liable to slip away from the instrument during the operation. If this happens after a pile has been excised, very inconvenient hemorrhage may take place. If, on the other hand, very great pressure is used, the parts are left bruised, and in a condition not adapted for rapid recovery.

It might be supposed that a thick blade would more effectually protect the subjacent parts in cases where the actual cautery is used; but experience proves that this is quite unnecessary. It seldom happens that after an operation of this kind the clamp is even warm to the touch. The thicker the blades, under similar circumstances, the less of the tumour can project beyond them, and the less, therefore, can be removed. In operating upon hemorrhoidal or other tumours, it is of considerable importance, as regards the subsequent pain and the rapidity of recovery, that the parts should not be bruised, and should not be dragged more than necessary out of their natural position. At different times I have tried a variety of instruments, and there is none, I believe, which will enable the surgeon to remove tumours in the way mentioned so safely and with so little disturbance to surrounding parts as that now described, made for me by Mr. Blaise, of St. James's Street. The rapidity with which patients recover, as compared with the operation by ligature, is a point of no little importance.

A general in the army, upwards of seventy years of age, had piles with protrusion of the mucous membrane, so that he could not walk without great inconvenience. He submitted to the operation without chloroform as I have described it. I saw him once or twice subsequently, but five days after the operation when I called I was told that, feeling well, he had gone to Brighton. This gentleman remains well up to the present time. If care be taken not to stretch or bruise the parts, and no slough be produced by the cautery, cicatrisation commences at once, and is soon completed.

In performing this operation I much prefer the heated iron to the galvanic cautery. When the iron is applied to the part at the heat mentioned, the cut surface will actually adhere to it. This probably depends upon the coagulation of the albumen in the tissues, and the same action that makes the parts

adhere in some degree to the iron will make them adhere to one another. The vessels in this way are sealed in a way that they cannot be where a slough is produced. With the galvanic cautery the temperature cannot be regulated, and it is difficult to say beforehand whether the wire will become of a black, red, or white heat. If a slough is produced, as it certainly would be by a red heat, that slough must separate before the process of repair commences. In operating on piles with the clamps formerly in use, I was in the habit of introducing a plug with a large bulb into the rectum, after the operation, in order to prevent any subsequent hemorrhage. This I have now discontinued, as unnecessary.

In advocating, as I do, the operation by clamp and actual cautery for the removal of solid piles or tumours in other parts. I would guard myself against being understood to mean that all piles should be treated in this way. Where the mucous membrane and the parts below are not much thickened, a very great deal may be done by constitutional treatment, ablution with cold water, and especially by the local use of suppositories containing the powder of cubebs. In cases, again, where these fail, the application of the strong nitric acid is an excellent remedy. The mode of applying this, as at first practised, was objectionable. The parts were made to protrude, and the acid was smeared over them; some of the acid is, under such circumstances, drawn up by capillary attraction between the folds of the bowel, and here it becomes mixed with the secretions of the parts, and, consequently, diluted. The result is, that it irritates but does not kill the mucous membrane with which it comes in contact, and a very considerable amount of inflammation and of suffering may be produced.

The best way of applying the acid, according to my experience, is to introduce a small speculum into the bowel, such as was first described by Sir William Ferguson. It has an opening on its side or near its extremity. It may be moved about until the pile projects into the opening. The pile is now wiped dry, and the acid applied on a glass rod. Several successive applications may be made in this way, but during the process the speculum should be held perfectly still. When the acid has produced the effect desired, some chalk-and-water should be freely used, so as to neutralise any remaining acid, and then the speculum may be removed. Performed in this way the operation is perfectly safe, and, as far as the acid is concerned, painless.

It is well to destroy one pile only at a time, but the operation may be repeated as often as necessary at an interval of three or four days.—*Lancet*, April 18, 1874, p. 541.

65.—TREATMENT OF HEMORRHOIDS BY LINEAR CAUTERISATION OF THE ANUS.

By Dr. WOILLEMIER.

After having written an exact history of all the methods of treating hemorrhoids, and after having either tried the greater number of them in private or hospital practice, M. Woillemier (*L'Union Médicale*) has finally fixed on the following rational procedure, as removing all dread of the operation both in the operator and in the patient.

The patient, whose rectum has been emptied in the morning by means of an injection, ought to be chloroformed; but if he prefer to remain awake, it is of little importance, as the operation only lasts some seconds. He is laid on the edge of a bed, with one leg extended, and the other bent as if he were going to be operated on for fistula. The assistant raises the disengaged buttock, the surgeon paints the anus and the surrounding parts largely with collodion, whilst an assistant, by means of bellows, drives off the fumes of the ether, which are sure to catch fire when a highly heated cauteriser is brought near them. During these preparations, two knife-shaped cauterisers have been placed in a small furnace, full of charcoal or burning wood. The blades of these cauterisers should be two centimètres long and one wide; the tip and edge should be blunt, as in ordinary cauterisers, but the back should be four or five millimètres thick, so as to hold enough heat. The surgeon takes one of these cauterisers when it is white-hot, and introduces it about one centimètre into the anus, bearing with the shoulder of the instrument rather more on the cutaneous than on the mucous orifice, and makes four cauterisation lines, before, behind, on the right and on the left. The operation is terminated when it has lasted five or six seconds. The patient is brought back to consciousness, and simple water-dressings only are applied to the anus. We must premise that, under the influence of the congestion produced by cauterisation, the hemorrhoidal tumour will reappear the first day or so, and sometimes larger than usual, but no notice must be taken of it. We can only relieve the pain of the patient, pain which has no relation to the cauterisation, by coating over the hemorrhoids with a narcotic ointment, and covering them up with a poultice. The tumour soon ceases to be painful, and is at last completely and spontaneously retracted. The time necessary for cure varies only according to the size of the hemorrhoids, the relaxation of the anus, and the age of the patient. It has never exceeded one month, and has sometimes been much less. In some subjects, even when circumstances have made success doubtful, cure has taken place as in simple cases. The patient

ought to be chloroformed, for, though the operation is rapid, it is also very painful, particularly in private practice, where the assistance is less efficient than in an hospital. The patient may struggle after one or two applications of the cautery, and even refuse to allow others to be made, so that the operation would remain incomplete. The orifice of the anus and the surrounding parts must be painted with collodion. This is a very important precaution. All surgeons have affirmed the difficulty of preventing the effects of radiating heat. To preserve the parts from these effects, cloths steeped in cold water and thin plates of wood have been used; but not only are these in the operator's way, but they are not, as a rule, efficacious. Collodion, on the contrary, even when applied in a thin layer only, forms an artificial epidermis scarcely permeable to heat and sufficiently protecting the skin.

It is necessary to dissipate the ether-vapour, or it would take fire as soon as the heated cauteriser is brought near the anus. The accident would not be of much importance, for the burning vapour is easily extinguished by blowing it out; but it is better to avoid it altogether. It is easy to understand the importance of the use of collodion in relation to the pain which succeeds the operation. The patient cannot feel pain in the points to which the iron has been applied, for the tissues are dead, but he suffers in the surrounding parts which have been attacked by the radiating heat, and the painful nature of superficial burns is well known. These burns, however, are not very serious, and only last about four days, at the time when the inflammation necessary for the falling off of the sloughs develops itself, or during defecation after the sloughs have fallen off. The cauterisers ought to be knife-shaped, or even with round points. To ensure the rapidity of the operation, they should be heated to white heat. One operation is frequently enough, but more than two are never necessary, how large soever the hemorrhoidal tumour may be, for we do not act directly on the latter, but on the anus.

In some cases, the tumour cannot be reduced before operation, or at least entirely so, when it still escapes outwardly in consequence of the involuntary contractions of the patient. No notice must be taken of this accident. The cauteriser is slipped between the tumour and the walls of the anus, for it is of little consequence if the hemorrhoids should be lightly cauterised by the back of the instrument.

Sometimes the shoulder of the cauteriser implicates the cutaneous circumference of the anus, but that is of no importance; it is even sometimes useful when the anus is considerably relaxed. There is no need to dread hemorrhage, for the cauteriser interferes only with the mucous membrane, the sub-

mucous cellular tissue at the entrance of the anus, and the skin at the edge of the orifice. At all these points the vessels are small, and when the hemorrhoidal tumour is touched by the back of the cauteriser, it is in so light a manner that no vessel of any importance can be opened.

If any accident is to be feared, it would be stricture of the rectum; but the four cicatrices which have been formed at the entrance of the anus, although possessed of great retractile power, are made linear in the direction of the intestine. Between them are intervals occupied by highly elastic tissue, which make stricture impossible. It may be objected that, if the anus remain sufficiently dilatable, the patient may have a relapse. This accident is certainly not impossible, but it is the business of the surgeon to estimate the state in which he finds his patient. If he be going to operate upon an old person having a large and old standing tumour, and whose anus has little resilient power, he should lean a little more heavily on the cauteriser, so as to implicate a greater thickness of tissues than in ordinary cases; and by this procedure he will be sure to avoid a relapse.—*Lond. Med. Record*, May 20, 1874, p. 303.

66.—ON THE TREATMENT OF HEMORRHOIDS, PROLAPSUS RECTI, AND FISTULA IN ANO WITH THE GALVANIC CAUTERY.

By THOMAS BRYANT, Esq., Surgeon to Guy's Hospital.

The surgical treatment of these affections of the anus and rectum has been much simplified by the introduction of the galvanic cautery, and cases of hemorrhoids and prolapsus recti, however well they may otherwise be clamped and cauterised, can with it be treated with greater facility, confidence, and success.

Cases of fistula in ano, by means of the same instrument, can likewise be divided in a simple and bloodless way, with less pain at the time than that caused by the knife, and less subsequent disturbance; indeed, I am disposed to think that in the bloodless nature of the operation for fistula in ano the general utility of the instrument is well displayed, although its special value is doubtless better seen in the treatment of the cases I have already brought before your notice.

The operation for the removal of hemorrhoids, or the cure of prolapsus recti, with the galvanic cautery, differs in no respect from that usually performed with the common cautery. The bowel to be treated is brought well down and into view either by means of the patient who is made to strain over a pan of hot water, or by an enema of warm water. The different

portions of the bowel to be removed are then seized and clamped in vertical pieces, radiating from the anus; each clamped portion, or rather the upper half of each portion, being then cut off with scissors. The parts are then made ready for the application of the cautery—the porcelain cautery heated to a red heat. This is to be rubbed over the surface of the projecting mass till it is burned down to the level of the clamp and turned into a dry eschar; each piece is to be dealt with in order, and each clamp removed as its segment is cauterised, the whole being carefully returned and pressed into the rectum when the operation has been completed. When the cautery is efficiently applied no bleeding follows this operation, and very little pain. The pain induced after the galvanic cautery is far less than that which follows the actual cautery. During the application of the cautery the soft parts should be well protected by strips of wet lint or ivory spatulas. The following cases will illustrate many of these points.

Case 1.—Internal Hemorrhoids; Removal by Galvanic Cautery; Recovery. (Reported by Mr. Hardie.)—Eliza D., aged thirty-six, married, came into Guy's Hospital under Mr. Bryant's care with extensive hemorrhoids, which had been gradually getting worse for three years. She had lost much blood from them, both when at stool and otherwise.

On Jan. 27th, Mr. Bryant applied a clamp to three distinct masses of hemorrhoidal substance, cut off the tops of the projecting masses, and freely burnt down the stumps with the galvanic cautery. He then removed the clamps and returned the projecting bowel; no bleeding following. The patient was under the influence of chloroform. Very little pain followed the operation. On the fourth day the bowels were relieved by an enema, and on the seventeenth she left the hospital quite well.

Case 2.—Hemorrhoids of Three or Four Years' Standing; Removal by Galvanic Cautery; Cure.—Wm. T. M., aged fifty-five, came under my care at Guy's Hospital with some large bleeding hemorrhoids of three or four years' standing. Nothing but their removal could be entertained. He had lost a great deal of blood at times.

On July 11th, 1872, the operation was performed, the piles having been previously brought down and secured with clamps. The projecting masses were cut off, and their bases well cauterised with the platinum cautery, dry eschars being formed. No bleeding followed, and but little pain. The man left the hospital, well, in three weeks.

Case 3.—Hemorrhoids for Thirty Years; Removal by Cautery; Cure.—Mrs. M., aged sixty-five, of Stratford, consulted me early in 1871 for the bleeding piles, with which she had been

troubled for thirty years. For the last ten she had suffered frightfully from them, being hardly able to stand or walk without their descent and loss of blood.

On March 27th, 1871, with the assistance of Dr. Drake, I removed the mass in four sections, each division having been clamped, cut off, and freely cauterised with the galvanic cautery. A steady recovery ensued, and the patient is now quite well.

Case 4.—Bleeding Hemorrhoids; Removal by Means of Clamp and Galvanic Cautery; Recovery.—In Feb. 1872, Miss L., a middle-aged lady, consulted me for a troublesome hemorrhoidal tumour, which had been steadily increasing for some years, and had been the cause of her losing much blood. On Feb. 16th, with the assistance of Mr. Jackson, of Highbury, I removed the mass in two parts by means of the clamp and galvanic cautery. The operation was almost bloodless, and was followed by rapid and complete recovery.

Case 5.—Hemorrhoids for Twelve Years; Removal by Clamp and Galvanic Cautery; Recovery.—Grace B., aged thirty-seven, came under my care in May 1872, for bleeding piles of twelve years' standing. On June 7th they were removed by means of the clamp and galvanic cautery in three masses. On June 13th the bowels acted painlessly, and by the 20th the parts had healed.

Case 6.—Hemorrhoids; Clamp and Cautery; Recovery.—(Reported by Mr. W. Brown.)—Henry E. O., a shoemaker, aged fifty-four, was admitted into Job Ward on March 18th, 1873, with an oval-shaped tumour situated at the orifice of the anus, about two and a half inches in length. It is divided into two parts: an outer, where the mucous membrane covering it is pale in colour and lobulated; and an inner, where the mucous membrane is red, very tender, and bleeds a little.

The patient had a chancre thirty-six years ago, but does not suffer from sore-throat nor from pains in his bones. About twenty-eight years ago, whilst fighting, he received a blow on the back in the right lumbar region, which gradually became painful and then swollen, and he was treated for it in this hospital by Mr. Cock; after this he remained quite well until seven years ago, when he noticed he had a tumour about the anus. He came as an out-patient, and it was tied and cut off. Three years ago he again noticed a tumour about the anus, which came down when he walked about, but he could easily reduce it.

On the 22nd March the patient was put under the influence of chloroform, and the inner tumour was clamped and burnt with the cautery. The folds of the skin about the anus were subsequently very œdematous and swollen, but by April 7th

these symptoms had subsided, and on the 14th he passed a motion without pain or bleeding, and he left the hospital. He returned to show himself a fortnight later, when he was quite well.

Case 7.—Hemorrhoids; Operation by Galvanic Cautery; Well. (Reported by Mr. G. W. Butler.)—John C., a photographer, aged twenty-nine, was admitted into Job ward on the 10th of November, 1873, with hard and enlarged glands in the neck, and piles, which bled when he passed a motion. His bowel also came down when he was relaxed. It appeared that he had suffered from bleeding piles for four years, and had been under treatment, but was advised to undergo an operation. A year previous to admission he contracted syphilis. A bubo came in his right groin, which was opened; and he had suffered subsequently from sore-throat and pains in his bones. He had been accustomed to drink hard.

Dec. 12. The hemorrhoids were brought down by placing the man over hot water. They came down in three masses, each of which was caught up by the clamp, cut off by a pair of scissors, and the galvanic cautery applied to the stumps. He complained for some days of pain after passing a motion, but there was no bleeding, and the external parts were quite normal. He was discharged from the hospital on the 22nd of December, cured.

Case 8.—Prolapsus Recti; Removal of Parts by Cautery; Cure.—In January, 1870, on the advice of Dr. H. Oldham, I was consulted by Miss L., aged fifty-seven, for a severe prolapse of the rectum, with daily bleeding from the part. It had existed for sixteen years, and been steadily getting worse. The bowel came down on the patient standing.

On Jan. 27 I removed four vertical portions of the mucous membrane of the bowel, having previously clamped them and cut off the projecting folds. No blood was lost at the operation, and a rapid cure ensued. The lady was up and about well in three weeks.

Remarks.—The cases just quoted are ample to illustrate the treatment of the more ordinary forms of hemorrhoids and prolapsus recti by means of the galvanic cautery; but under exceptional circumstances some modifications of this treatment may be called for. Thus, in cases of piles as represented by the growth of small vascular strawberry-looking tumours, I have been content with the direct application of the cautery to the growth, having exposed it well by means of a speculum; and I have found this practice a good one. In other cases, again, of prolapsus recti, I have simply scored obliquely in lines the surface of the mucous membrane of the prolapsed bowel, and returned it; and this treatment has been followed by good success, more particularly when employed in the milder forms of

the affection. In cases of long standing such treatment is inapplicable. Nothing but the removal of portions of the prolapsed mucous membrane will effect a cure.

With these remarks upon cases of hemorrhoids, I will pass on to consider the treatment of fistula in ano by the galvanic cautery; and I am pleased to say it is as simple as it is satisfactory. It is performed as follows. The platinum wire, or twist of platinum wire, is first introduced through the fistula, and I generally do this through the groove of the probed director when it has been made to pass through the fistula into the bowel. I then hook, with the finger that is in the rectum, the end of the wire downwards through the anus, and remove the grooved probe, in this way one end of the wire being made to protrude through the rectal orifice of the fistula, out at the anus, and the other through the external orifice of the fistula. The two ends of the wire are then connected with the poles of the charged battery, and the wire heated, the division of the fistula being made either by means of the *écraseur*, or a gentle sawing movement of the wire, or traction upon it. The wound may then be dressed with oiled lint or cotton-wool, and the case treated upon ordinary principles, the wound having simply to heal by granulation. In the following cases the above practice is illustrated.

Case 9.—Fistula in Ano Treated by Cautery; Cure.—Jonah C., aged forty-six, a delicate man, was admitted into Guy's Hospital under Mr. Bryant's care in May, 1869, with a fistula in ano. It had followed upon an abscess three months previously. On May 20th Mr. Bryant divided the fistula by means of a platinum wire heated with the galvanic battery. The operation was done with facility, and was quite bloodless; but very little pain followed the operation. The wound healed rapidly, and in less than a month the man left the hospital cured.

Case 10.—Fistula in Ano: Division by Galvanic Cautery; Recovery.—Thomas D., aged forty-three, came under Mr. Bryant's care at Guy's Hospital in August, 1872, with a fistula in ano. It had existed for five months. On August 6th Mr. Bryant laid the fistula open into the rectum by means of the galvanic cautery. He passed a probe first through the fistula, and then drew the platinum wire through the sinus, one end of the wire being brought out through the anus and the other through the buttock. The wire was then heated, and by means of a gentle drawing motion the tissues were divided. No bleeding or pain followed the operation. On the fourth day the bowels acted, and in three weeks the parts had completely healed.

Case 11.—Fistula in Ano ; Division of Sphincter with Wire of Galvanic Cautery ; Cure. (Reported by Mr. Parker.)—James S., aged fifty-eight, was admitted into Guy's Hospital on June 1st, 1870, with a fistula in ano of four months' standing. On June 20th Mr. Bryant divided the fistula by means of the wire heated with the galvanic cautery. He first passed a director through the fistula into the bowel, and the wire through the groove of the director, and having in this way threaded the fistula, he simply, by a gentle drawing motion, made the wire burn its way out, having heated it by connecting the two ends of the wire with the battery. An excellent recovery followed. Not a drain of blood was lost during the operation, no dressings were required after it, and a very insignificant amount of pain was experienced.

Mr. Bryant has now treated many fistulæ in this way, and thinks highly of it in patients with the hemorrhagic diathesis, or in others where any loss of blood is to be avoided.

Case 12.—Fistula in Ano ; Division by Galvanic Cautery ; Recovery. (Reported by Mr. Stuart.)—John S., aged forty-four, a cellarman, was admitted into Guy's, under Mr. Bryant's care, on July 8th, 1869, for fistula in ano. It had existed for eighteen months, and had followed an abscess. The external opening was on the right side of the anus, about one inch and a half from the orifice, and the inner opening was about one inch up the bowel.

July 17th. The director having been passed through the fistula into the bowel, and its end made to protrude through the anus, Mr. Bryant passed the platinum wire of the galvanic cautery along the groove, and, having attached its end to the two poles of the battery, by a slight sawing motion made it cut its way out. Little or no pain followed this operation, and on the third day the patient was up and dressed; in two weeks the part had healed.—*Lancet*, May 16, 1874, p. 681.

67.—THE TREATMENT OF HEMORRHOIDS BY THE INJECTION OF THE TINCTURE OF PERCHLORIDE OF IRON.

By WILLIAM COLLES, Esq., Surgeon to Steevens' Hospital, Dublin, and Lecturer on Surgery in the Hospital College.

It is remarkable that, in a disease of such constant occurrence as hemorrhoids, surgeons should still differ as to the nature and origin of the tumours—some considering them as dilated veins, and others regarding them as mere growths of a vascular tissue. And this difference of opinion has much influence on the treatment of the disease.

From my observation on examining the part when removed,

and also considering the nature of the bleeding, which is generally bright arterial blood, and that at times the orifice of a minute artery can be seen pulsating, I have been led to consider them as a vascular growth resembling *nævus* in children, or erectile tissue in adults.

For the treatment of this disease the surgeon has many plans of treatment—either by medicines used locally or constitutionally, or by various operations. Still, we observe, the advocates of each plan will tell us that they have met cases which resisted the treatment that others advocate as infallible, and that theirs is the best. I beg to add another proceeding in these cases, not to supersede all others, but still, I think, presenting many advantages, and more likely to be generally successful, and also less liable to dangerous consequences than some others. Some years ago I saw the late Mr. Cusack treat the disease by the introduction into the centre of each tumour of a needle coated with nitrate of silver. I have also often tried this cure; but it causes considerable pain for some days after the operation.

I then thought the same effect would be obtained, with little or no pain, by acting on the internal structure, and treating them, as we do *nævus*, by the hypodermic injection of some preparation of iron or other ingredient, in order to excite a certain amount of inflammation, and secure the coagulation of the blood in the minute vessels composing the growth, and then subsequent absorption; and such a procedure I adopted in the following case:—

Case.—(Reported by Dr. F. W. Warren, Resident-Surgeon to Dr. Steevens' Hospital.)—Denis R., aged forty-one years, of delicate and anæmic appearance, was admitted to Dr. Steevens' Hospital, March, 1874, suffering from bleeding internal piles.

Previous History.—He stated that for the past twelve years he had suffered from irritation about the anus, with a feeling of heat and fulness in the parts, these symptoms undergoing exacerbations at intervals, when an attack of bleeding would give him temporary relief. This state of things continued until a few months ago, when the attacks became more frequent, the hemorrhoidal tumours protruding during each act of defæcation, accompanied by hemorrhage more or less severe. He also stated that the bowels were habitually constipated, requiring purgative medicine to keep them regular.

Symptoms on Admission.—The patient presented a very blanched and emaciated appearance. He complained of a sensation of itching and smarting about the anus, increased to pain during defæcation with considerable hemorrhage. He stated that he was daily becoming weaker, and presented all

the appearance of one suffering from repeated attacks of hemorrhage. Appetite very poor. On examination (the patient being desired to force down), three tumours (each being about the size of a walnut) protruded, and became visible outside the anus. These tumours presented the usual smooth red appearance. These projections disappeared spontaneously, or with gentle pressure, after defæcation. Ordered confect. sulphuris, a teaspoonful three or four times daily, so as to combat the habitual constipation, and infusion of rhatany to be applied locally.

March 27th. Feels somewhat better; the irritation in the anal region being diminished. Bowels regular. As the piles still protrude during defæcation and bleed, an enema of warm water was to-day administered, this causing the hemorrhoids to protrude; fuming nitric acid was then freely applied. This procedure caused excruciating pain which continued for two hours.

March 31st. Much in the same condition. States that he does not feel anything better. Had one motion from the bowels since the application of the nitric acid, accompanied by great pain and some hemorrhage. Bowels confined. Ordered haust. oleosus.

April 11th. Has not experienced any relief to his symptoms, the mucous projections still protruding during defæcation, accompanied by hemorrhage and pain. To-day, the piles being forced down, the ordinary tincture of the perchloride of iron was injected, by means of a hypodermic syringe, into each hemorrhoidal tumour (the quantity injected into each being about 20 minims). The patient was then removed to bed. The injection caused much less pain than the nitric acid.

April 18th. Expresses himself much better. Has had no bleeding since, and much less pain during defæcation. Ordered suppositoria acidi tannici—one to be introduced into the rectum each night.

April 28th. Greatly improved. The hemorrhage, pain, and irritation have completely subsided. Presents a much better appearance. Appetite improving daily. Bowels regular.

May 9th. The rectum was to-day explored by means of a speculum, and no trace of the piles could be discovered, except three nodules of cuticle, each the size of a shrivelled currant. —*Dublin Journal of Medical Science, June 1874, p. 505.*

68.—DESCRIPTION OF A CATHETER URETHROTOME WITH CONDUCTING BOUGIE.

By W. F. TEEVAN, Esq., B.A., Surgeon to the West London Hospital and to the St. Peter's Hospital.

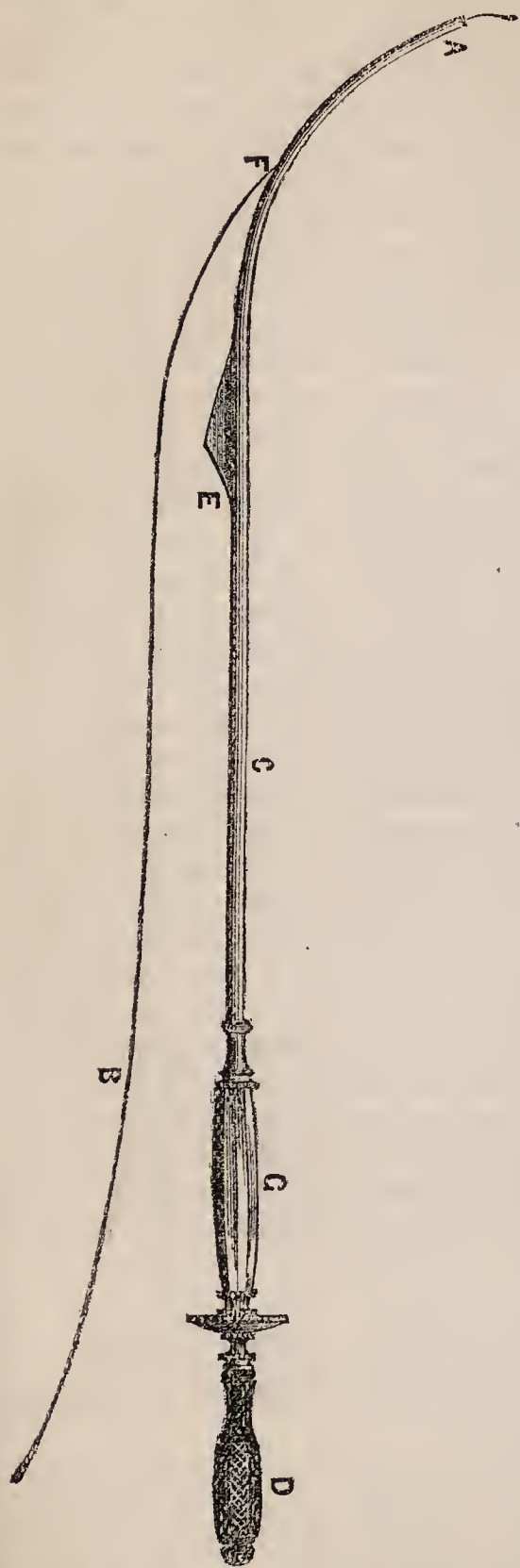
Of the countless number of urethrotomes which have been invented, but few remain in use at the present day, and even to the majority of the survivors exception must be taken, as they are wanting in two important particulars. Firstly, when passed up the urethra they do not proclaim their situation; they go somewhere, it is true, but the locality may be the rectum, false sac, or bladder. A distinguished surgeon performed urethrotomy to his entire satisfaction, passed a large instrument after the operation, and left the patient progressing most satisfactorily. But a short time had elapsed when the surgeon was surprised to see his patient driving up to his house, and on enquiring the object of his visit, he ascertained, to his chagrin, that the patient had come to demonstrate to him that he had performed recto-urethrotomy. Then, again, most of the urethrotomes are so large at their vesical extremity that they cannot be used unless the urethra have previously been dilated up to a certain calibre. This remark especially applies to those urethrotomes that divide from behind forwards. For a urethrotome to be a safe and efficient instrument for the division of tight strictures in the deeper portion of the urethra, it must possess two important qualities. Firstly, its vesical extremity must be so fine that it can be introduced through very tight strictures. It consequently follows that the urethrotome must be one which divides from before backwards; for if the stricture be capacious enough to permit an instrument to pass which cuts from behind forwards, the indication for any operation is doubtful, for if we have dilated the urethra up to half its natural calibre, why should we not persevere with the treatment which has been so successful, and that, too, in the worst and most troublesome stage of the complaint? What we want is to save time in certain cases where patients have, for instance, to go to sea suddenly, and where time does not permit of a course of treatment by gradual dilatation. We therefore require a urethrotome which is capable of setting to work immediately the smallest bougie has been passed. Secondly, it must proclaim its position unmistakably. Now, most urethrotomes lack one or other of the above essentials. Of those instruments which divide from before backwards, two stand out prominently—i.e., the urethro-

tome which was invented by M. Bonnet, of Lyons, and the one which bears M. Maisonneuve's name. Of the first one it may be said that it lacked the first quality which I have indicated as necessary, but possessed the second one, whereas the converse might be stated of the second urethrotome.

Five years ago a great advance was made in the construction of the urethrotome by Professor Gouley, of New York, who *tunneled the vesical end of his instrument, so that it could be slid over and along the finest bougie already passed*. So far as I can ascertain, no English surgeon had ever heard of such a thing as a tunneled catheter or urethrotome until it was sent to him from the other side of the Atlantic, and I consider that Professor Gouley's invention marked an era in the surgery of the urinary organs. He, for instance, has converted that dumb and consequently unsafe instrument which is called "Syme's staff" into a *conductor which tells the surgeon where it has gone to*, so that external urethrotomy can now be performed with a certainty hitherto unattainable, and also extended to desperate cases which were formerly beyond its reach.

In the *Lancet* for July 5th, 1873, I gave a description of a catheter with a conducting bougie founded on Professor

Gouley's principle, but differing in some respects from his; and I shortly afterwards conceived the idea of adapting the vesical



third of the instrument to the urethrotome, in order to endow it with safety and a more extended sphere of usefulness. The instrument is composed of a very slender olivary whalebone bougie (see woodcut, B), not so large as a No. $\frac{1}{4}$, which is first of all introduced, or wriggled I would rather say, into the bladder apparently, when the catheter (A C) with a handle (G) is slid over and along the bougie (B), which emerges from the catheter through the slit which extends from the handle to the point (F). If the bougie have been passed successfully into the bladder, urine will of course flow from the end of the catheter; but if it should not do so, the catheter may yet be in the bladder, for the slit may have permitted some blood or mucus to block up the tube, and it is therefore necessary to pass the bougie through the whole length of the catheter to clear its interior. When it has been proved that the instrument is in the bladder, the knife (E D) may be slid along the catheter firmly and slowly, and the stricture divided. The apex of the blade is blunt, so that it will usually keep the wall of the urethra away from the anterior side of the triangular knife, which is the only edge that cuts; whereas Maisonneuve's blade cuts going in and coming out, thus giving rise to M. Civiale's objection that the former surgeon's urethrotome "promenaded" the urethra, cutting here and there. If the surgeon consider that the urethra upon which he is about to operate will resent the introduction of the knife and embrace it tightly, it is well then to sheathe the cutting edge with some wax, so as to protect the healthy urethra; or, what is still better, to adapt Mr. Donald Napier's recommendation as to the use of cacao-butter. It would thus appear that the instrument I have described realises two great desiderata—safety and an extensive sphere of usefulness. Inasmuch as the vesical extremity of the catheter tapers to the size of No. 1, English, there are but few strictures to which it is not applicable; and as the surgeon does not use the knife till he has *proved* that the catheter is in the bladder, it will prevent a repetition of such *petits désagréments* as promenades into a false passage or the rectum—*Lancet*, May 23, 1874, p. 724.

69.—URETHROTOMES FOR NARROW STRICTURES.

By BERKELEY HILL, Esq., Surgeon to University College Hospital.

In the *Lancet* for May 23rd [see preceding article,] Mr. Teevan describes an instrument he has devised for cutting narrow strictures. It is Maisonneuve's urethrotome introduced in a manner different from that adopted by Maisonneuve himself. As Mr. Teevan appears to be unaware that I showed to

the British Medical Association at its congress in August last a urethrotome for cutting very narrow strictures, the mode of introducing which is precisely that employed by Mr. Teevan, I venture to present a comparison of the two instruments, as I believe mine to be free from the objections that have led many surgeons to discard Maisonneuve's instrument for some years past. Before doing so, I may remark that believers in cutting or splitting strictures of certain kinds may congratulate themselves that Mr. Teevan, who has not infrequently advocated gradual dilatation by flexible bougies as the sole safe and efficient method, has at last turned his attention to cutting. However, this by the way. The contrivance for introducing this urethrotome—namely, by slipping its point, which is perforated by a "tunnel" or eye, along a whalebone bougie previously passed through the stricture, is attributed by Mr. Teevan to Dr. Gouley, of New York; I believe, however, that Dr. Van Buren, the eminent surgeon of that city, whose assistant Gouley was, is really the originator of the idea, though it has been adopted and perfected by Dr. Gouley. Whoever is the inventor, the plan has been familiar to those who give attention to urethral affections for some years, and being no novelty, I laid no stress upon it when describing my urethrotome to the Association.

The objections to Maisonneuve's mode, which is adopted by Mr. Teevan, of applying the cutting edge to the stricture, are—first, that the sharp edge of the incisor is passed down the urethra without protection. It is true that the healthy elastic part escapes, or is only slightly wounded, by the cutting edge, and the amount of mischief produced is usually not serious. But the second defect is most important; the knife attacks the stricture from before backwards, and it is almost impossible to prevent the stricture from being pushed back along the staff, and only partly divided, so that when the partial incision has healed the stricture is as bad as ever. Mr. Teevan suggests that the healthy part of the urethra can be shielded by covering the knife with cocoa butter, and this may be a sufficient guard; but the main objection still remains, that of cutting from before backwards—a plan which has been proved over and over again to be quite unsatisfactory. Bearing this in mind, I was able, by Mr. Coxeter's ingenuity—for this part of the instrument is entirely his,—to get a mode of passing the cutting edge through the stricture *before* the incision commenced, and thus cutting from behind forwards, or from the bladder towards the meatus. The description of my instrument was published as part of the proceedings of the surgical section of the British Medical Association in the Journal for Nov. 29th, 1873, and is briefly as follows:—A whalebone bougie, less than one-

fourth of No. 1 catheter in diameter, is passed through the stricture to the bladder; along this the tunneled eye of the urethrotome is threaded, and the instrument conducted down to the stricture, and through that to the bladder. If the operator is in doubt about the position of the bougie, he may reassure himself by running a No. $\frac{1}{2}$ catheter along the whale-bone guide, and, having allowed urine to escape, withdrawing the catheter, leaving the guide *in situ*, on which to pass the staff. When the staff is passed fairly through the stricture, a slender rod, carrying a knife at its end, is pushed along the interior of the staff until the point of the knife reaches a short incline, which turns it out against the wall of the urethra just behind the stricture (see A in the woodcut, *Retrospect*, vol. lxix., p. 252), the limits of the stricture having been previously ascertained by a manœuvre of a very simple kind. When the knife is in place, the whole instrument is drawn forwards, and the cutting edge is carried along the floor of the urethra to any depth required short of half an inch, the depth of the incision being regulated by a screw at the handle. Thus the knife is wholly contained within the staff until it is projected therefrom behind the stricture, and can be drawn in again as soon as it has traversed the stricture. Nor is the instrument large: at its greatest width it is less than a No. 2 catheter, and at its point is considerably smaller. Mr. Teevan does not mention the calibre of the staff part of his urethrotome, though he states that the curved end has the thickness of a No. 1 catheter. But the straight grooved part which carries the Maisonneuve's knife cannot be much less than a No. 2 catheter. Mr. Teevan takes credit for his instrument that it does not, like Maisonneuve's, cut on coming out as well as when going in. Now Maisonneuve at first did not make his instrument cut while being withdrawn, and added this power in order to supplement the imperfect dividing action of the knife when only pushed against the stricture. Thus Mr. Teevan's modification is still open to Civiale's objection that the Maisonneuve knife "promenades the urethra, cutting here and there;" it only cuts a little less in its "promenade" than does the more finished and elaborate instrument of Maisonneuve.

Having pointed out that while equally adapted for narrow strictures, our instruments differ in the mode they incise, and, as I believe, much to the advantage of my own, I should like to add a few words on a difficulty in using the guide-bougie and tunneled catheter which longer experience has taught me, but which I have not yet succeeded in altogether overcoming. I am led to do this because neither Dr. Gouley nor Mr. Teevan mentions it. Indeed, the reader who has not tried to pass tunneled catheters or guide-bougies would suppose the pro-

ceeding to be a most simple one. This is the difficulty. Old strictures are often very irregular or tortuous internally, and it is no easy matter, sometimes, when they are also very narrow, to "wriggle," as Mr. Teevan well expresses it, the stiffish whalebone through the stricture; a limp bougie will not serve for a guide. Nevertheless, this is perhaps only a question of time and patience. But, worse still, the tunnel of the staff is occasionally so locked on the whalebone in the tortuous stricture, that it cannot be pushed along with any justifiable amount of force. I have once cut the whalebone short off at the stricture, leaving half in the deeper part of the urethra, where it remained for two or three days, until its presence widened the stricture sufficiently to allow the stream to wash it out. No harm ensued, and the urethra was dilated without difficulty, but the accident was at the least not pleasant to the operator. On another occasion I split up the whalebone into long shreds by trying to force the catheter along it. To avoid this, when there is difficulty in passing the staff along the guide I content myself with tying in the guide for one or two days, till its presence has smoothed or widened the narrow part, and the staff will slide through easily. I found by inquiry of my friend, Dr. Bumstead, when he was in England last summer, that the New York surgeons also thus evade the difficulty. But one must confess that this necessity very materially lessens the brilliancy and rapidity of the mode of treatment.—*Lancet*, June 13, 1874, p. 830.

70.—LANDING-NET FOR REMOVING A STONE OR STONES FROM THE BLADDER.

By RICHARD DAVY, Esq., Surgeon to the Westminster Hospital.

During the operation of lithotomy, I have been often hurt to see a stone crushed by removal by the forceps (and thereby a specimen lost), or have waited expectantly to see fragments removed by the scoop; so I venture to suggest that we may gain a wrinkle from fishermen, and use a net for extractive purposes.

The net (of silk or canvas, according to the size of the stones) is made to slide on a curved wire, bent on the principle of midwifery forceps. After the cut has been made into the bladder, the net is introduced through the wound to the side of the stone or fragments; and, by gently tickling the stone and manipulating the net, the stone or fragments (coaxed in) are caught on withdrawal. These stones *plus* silk are the occupants of the perineal wound instead of stone *plus* forceps or scoop. From the dead bladder I have removed stones fifty times by the net;

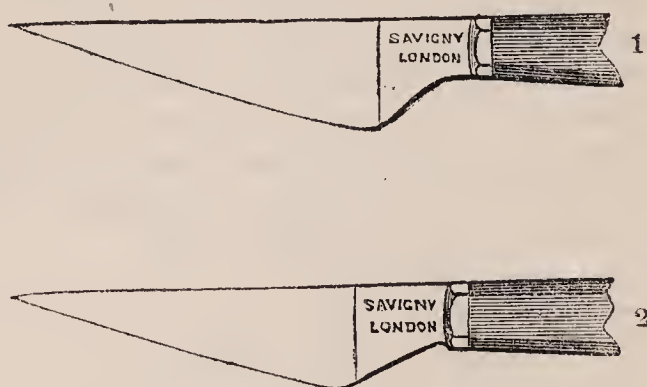
yet, useful as this net is, it is not so important an ally as the law of gravitation. Savigny and Co. will show this net to surgeons.—*British Med. Journal*, June 27, 1874, p. 842.

AFFECTIONS OF THE EYE.

71.—A MODIFIED CATARACT KNIFE.

By JABEZ HOGG, Esq., Surgeon to the Royal Westminster Ophthalmic Hospital, &c.

The size and form of knife used in making the section in the operation for cataract is a matter of more importance than may at first sight appear. During my early acquaintance with eye surgery, and for many years after I became connected with the Royal Westminster Ophthalmic Hospital, the knife employed by preference over that of any other was the triangular knife, known as "Beer's knife. Mr. Guthrie looked upon it as the



Figs. 1 and 2, Beer's Knife

perfection of a knife; for my own part, and perhaps from long handling of small delicately-balanced knives in microscopical dissections, I always thought it rather too broad and too large, especially towards the base; the angle it subtends is decidedly too great. Three or four years since, on reading a paper before the Medical Society of London, I exhibited a modified narrow-bladed cataract-knife to the Society. The knife was something between a small Wenzel and a narrow-bladed Graefe, and at the same time I pointed out what I considered the defects of the latter, namely, its extreme thinness and its liability to bend if made to traverse a cornea somewhat degenerated, one undergoing arthritic change, *arcus senilis*. I notice also, that when Von Graefe's thin knife enters the cornea too obliquely, the point may pass through the lamellæ some distance without entering the anterior chamber, and thus miss making a counter

puncture exactly where we wish it to be. The success of the operation is occasionally somewhat marred in this way, for the corneal section will be too small to admit of an easy delivery of the lens. This is, however, an accident which does not often occur; nevertheless, for the reasons I have given, I requested

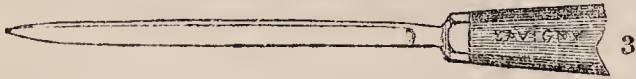


Fig. 3, Graefe's. Fig. 4, Wenzel.

Messrs. Louis Blaise & Co., of St. James's Street, to make me a knife, a *modified* Graefe, the blade of which is stouter than his, and in shape partakes of Wenzel's, being of course very much smaller. I place it for comparison, side by side with those of Graefe and Wenzel. I believe those who may honour me by



Fig. 5, Modified Graefe Knife.

making trial of this knife will be satisfied with its superiority. One word with regard to the speculum employed. I have so often found the patient, by the action of the muscles, able to close the eyelids during the operation, that to obviate this I have had a screw collar arranged to act upon the jointed

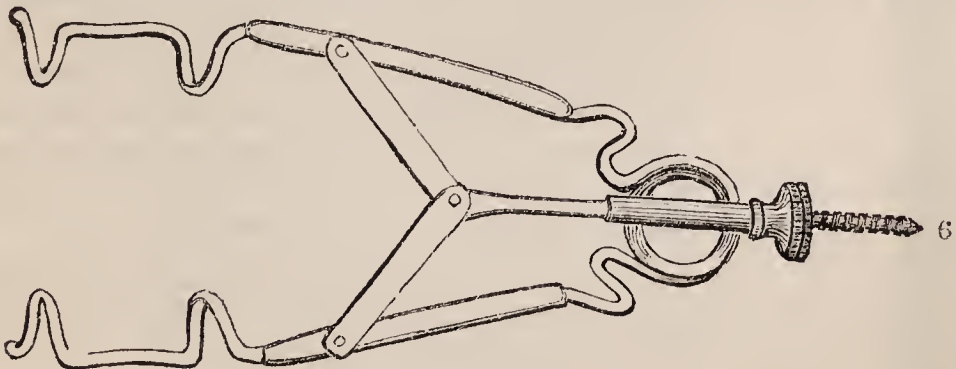


Fig. 6, Speculum.

crossbar; this gives a steadiness and amount of resistance which will be found of value in operations of the eye.—*Medical Press and Circular*, April 29, 1874, p. 351.

72.—A NEW FORCEPS FOR TEARING THROUGH OPAQUE CAPSULE.

By CHARLES HIGGENS, Esq., Assistant Ophthalmic Surgeon to Guy's Hospital; Surgeon to the Central London Ophthalmic Hospital.

The instrument depicted in the woodcut is made for the purpose of tearing through those dense opaque membranes not unfrequently met with occupying the pupillary area of an eye after cataract extraction, or more commonly when the lens matter has been removed by absorption following upon an injury or operation.

One of the figures shows the whole instrument; the other, representing only its lower extremity, is introduced expressly to show the arrangement of the teeth (A). These project from the lower surface of each branch vulsellum-like, and will seize and tear through anything, however tough and tensely stretched across the pupillary area, leaving a good clear opening where the densest membrane previously existed.

The branches of the instrument, together with its shank, are gilded as far as (c), in order to make it plainly visible when introduced into the anterior chamber.

Any dragging operation upon opaque capsule, as is well-known, is fraught with the greatest danger to the eye; nevertheless we are bound to make an attempt to restore vision in spite of risks. It is for cases that cannot be remedied by the use of needles or other means that these forceps are designed; and I venture to think they will be found to answer their purpose admirably.

The instrument is made by Messrs. Krohne and Sesemann, 8, Duke Street, Manchester Square.—*Lancet*, August 8, 1874.



73.—ON THE TREATMENT OF CATARACT PREVIOUSLY TO OPERATION.

By BOWATER J. VERNON, Esq.,

(Being some remarks upon a paper by Mr. CRITCHETT.)

In the *Annales d'Oculistique* (Sept. and Oct., 1873) are some valuable notes on the question, one not very fully treated of in standard works: Is it the duty of the surgeon to warn his patient of the nature of the disease when it has but barely commenced, and when a long and anxious time must elapse before any operation can be attempted? Mr. Critchett, even at the risk of appearing to overlook the true nature of the case, would be reticent, knowing how the thought of impending blindness from cataract is dreaded.

Another question which has to be met is, whether there is any cure, any means of removal by medicine or otherwise, short of an operation. It is at this time that patients with cataracts are apt to fall into the power of quacks and charlatans. Though the answer to this question must be a decided negative, yet there is much that may be done to relieve the present discomfort; for instance, the amount of light may be regulated by the employment of proper glasses, and by the continued use of atropine in solution. It will be well too that the patient should be seen at regular intervals, in case the commencing cataract may be the precursor of some more serious condition, such as glaucoma; and by degrees the knowledge that the increasing dimness of vision is due to cataract and to nothing worse, may in itself be a reason for looking hopefully to the future.

The time for operative interference has arrived when—

1. The cataract is completely matured;
2. The lens of the second eye is also so opaque that the patient is on the point of having to relinquish his occupation.

The social position of the patient must be borne in mind; but the second condition is to be insisted upon, because surgeons of the old school used to delay till blindness was complete, and there is a tendency amongst some surgeons of the present day to adopt the other extreme and to attack every single cataract at once.

There are some cases of cataract which will, Mr. Critchett thinks, always give rise to difference of opinion, and which indeed are embarrassing; viz., when there is very great impairment of vision while the cataract is yet far from maturity. Is the surgeon to operate upon an immature lens? or is he to hasten the maturity by some operation? The latter course Mr. Critchett believes to be full of danger; and, except in cases where the cortical layers of the lens are much involved it is, he

thinks, the duty of the surgeon and in the patient's interest to wait. In the cases where one is cataractous and the other quite sound, Mr. Critchett thinks that in young subjects it is well to operate, but in elderly patients, while one eye retains its perfect power, the cataract in the other should not be interfered with.—*London Medical Record*, May 20, 1874, p. 310.

74.—THE OPERATION OF REPOSITIO CILIORUM FOR TRICHIASIS.

By Dr. D. ARGYLL ROBERTSON, Ophthalmic Surgeon to the
Royal Infirmary, Edinburgh.

The operation of repositio ciliorum, also termed illaqueatio, was known to Celsus, and by him carefully described in his well-known work. Celsus himself, however, expresses an unfavourable opinion of the operation; and this appears to have been shared by his contemporaries, as this method of treating trichiasis fell into disuse, and was completely abandoned until about twelve years ago, when Dr. Snellen of Utrecht re-introduced it. Notwithstanding Dr. Snellen's advocacy, the operation has not been very extensively adopted, and is scarcely referred to in any of the standard works on diseases of the eye. As it is, in my opinion, an operation of great value when applied to suitable cases, I have deemed advisable to direct again the attention of the profession to it. The principle of the operation consists essentially in causing the offending eyelashes to be mechanically turned away from the eye, and made to grow more or less in the proper direction by making them pass under a narrow bridge of skin. The following is the method of performing the operation:—A very fine curved needle has the two extremities of a very fine waxed silk ligature (or hair, as Celsus directs) passed through its eye. The needle, being firmly grasped by suitable forceps, is then passed through a narrow fold of skin at the very margin of the lid, close to one of the inverted eyelashes. The point of introduction should be external to the point of emergence of the eyelash, but as close to it as possible; and the needle should be brought out after passing about $\frac{3}{4}$ " or 1" under the skin. The needle and ligature should be drawn through until a small loop alone remains, when, by means of a fine pair of forceps, the eyelash is passed through the loop. Traction is then made on the ligature, and the loop with entangled eyelash, is drawn through the tunnel of the skin.

The other misdirected eyelashes are similarly treated. The chief difficulty in the performance of the operation consists in getting the eyelash entangled in the loop of the ligature, as

the bleeding which occurs from the points of puncture causes the eyelashes to be matted together, or to adhere to the ligature, and the misdirected eyelash is almost always enveloped by a small clot. When this source of annoyance is troublesome, it is advisable to wait a minute or two after the passage of the needle before proceeding to the ensnaring of the eyelash. In some nervous patients, I have found it advisable to introduce the lid-forceps or clamp, so as to stretch the lid and enable it

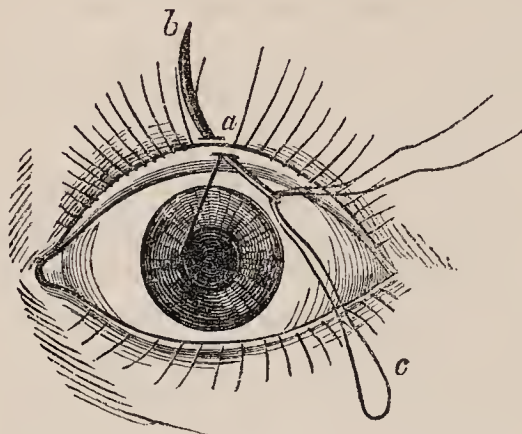


Fig. 1 represents the passage of the needle, armed with the ligature, under a band of skin close to the point of emergence of an inverted eyelash. (The direction of the needle has been represented a little oblique, not to complicate the diagram.

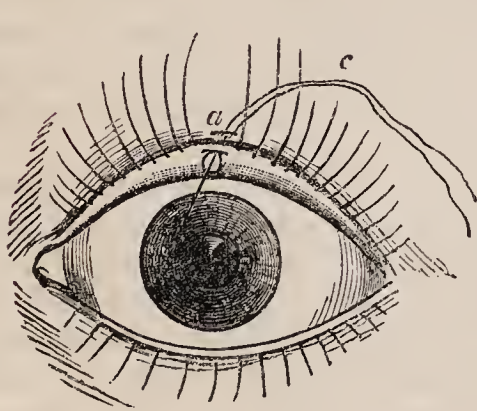


Fig 2 represents the misdirected eyelash involved in the loop of the ligature.

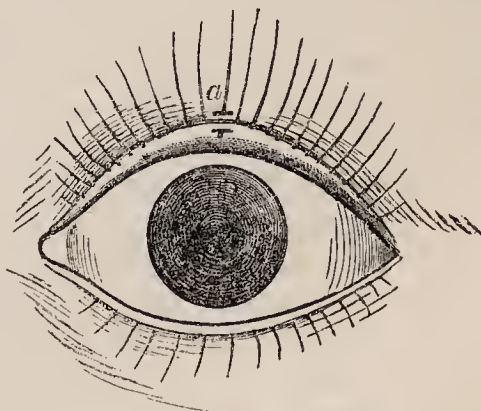


Fig. 3 represents the eyelash in its altered position.

to be more readily fixed, and thus ensure accuracy in the points of introduction and emergence of the needle. One thing that must be carefully attended to is to make the point of introduction of the needle as close to the offending eyelash as possible, as, if it be made at a little distance, the resiliency of the hair causes it gradually to emerge from the tunnel and resume its former position.

Very little irritation follows this operation, and no special

after-treatment is necessary. The patient, however, must be warned against touching or rubbing the eyelashes for twenty-four hours after the operation, as he might thus force the eyelashes back out of their new channel.

My attention was first directed to this operation by a short notice of a paper on it by Dr. Schulek, in the *Annales d'Oculistique* for 1871; and, since then, I have performed it in about a dozen cases. In all it has been attended with excellent results, except the first time I employed it, when I made the mistake of using too coarse a needle, and also attempted to pass several of the eyelashes through one of the tunnels. The result was, that most of the eyelashes freed themselves from restraint, and returned to their original position. I subsequently operated on this case with success.

On examining cases that have been successfully operated on after the lapse of several months, no eyelashes are found passing under bridges or bands of skin, and yet all the eyelashes are properly directed. I have not yet been able to satisfy myself of the process by which this state of matters is attained. In some cases, particularly where the bridge of skin under which the eyelash has been passed is very thin, I believe the eyelash ulcerates its way through; in others, again it is possible that the eyelash gradually slips back out of the tunnel, but, having been for some time kept in a proper direction, has a normal curvature given to it, and a proper direction imparted to the follicle which produces it, which direction they retain. I believe however, that in the majority of instances, the eyelashes after a time are shed, and fall out of the tunnel, and the new ones that are generated grow in the normal plane.

This operation is, unfortunately, only applicable to those cases of partial trichiasis in which the eyelashes inverted are few in number and of considerable size. In such cases it is, I am convinced, much superior to any other method of treatment employed at the present time. Where, however, the trichiasis is general, this method of operating would be excessively tedious and ineffectual, owing to the fineness of some of the inverted lashes. Where, on the other hand the misdirected hairs are few and fine, the operation described is inapplicable, as the difficulty of getting them entangled in the loop of the ligature is much increased; and, even if successfully accomplished, and the eyelashes drawn into the tunnel, they are very apt to free themselves, not being sufficiently firmly held in their new position.

The reason why the operation in ancient times fell into disuse, is evidently to be found in the fact that it was then employed in all forms of trichiasis; and, as suitable cases form only a comparatively small proportion of the whole, the general results of the operation could not have been satisfactory.

To Dr. Snellen great credit is due for several ingenious operations upon the eyelids he has devised, as well as for the re-introduction of this old operation.—*Edinburgh Medical Journal*, May, 1874, p. 988.

75.—ON OPENING THE CAPSULE BEFORE MAKING THE
CORNEAL SECTION IN THE OPERATION FOR
SENILE CATARACT.

By SPENCER WATSON, Esq., London.

One step of the operation for extraction of senile cataract—the theoretically simple process of opening the capsule—presents some difficulty and not a little danger. However fully dilated the pupil may have been before making the corneal section, no sooner is this step completed than the aqueous, if it has not already escaped, rushes out, and the pupil contracts, the iris coming of necessity in contact with the cornea. Supposing the operation to be Graefe's, the next step is to excise a piece of iris, and then comes the laceration of the capsule. The removal of a piece of iris facilitates this step, but at the same time makes it more dangerous. It opens the space for the admission of the cystitome, and allows it to have a wide range of action without coming in contact with the iris; but it also much increases the risk of dislocating the lens and causing prolapse of the vitreous. If the capsule happen to be tough, and the suspensory ligament weak and friable, this accident is very likely to happen; and especially if the fixing forceps is being used, and the patient is under an anæsthetic. If, however, the operation is the old flap or any other operation not necessitating the removal of a piece of iris, the difficulty of lacerating the capsule freely without bringing the cystitome into contact with the iris is almost insurmountable. But this is not the only danger. The point of the cystitome must, in order to make a free opening of the capsule, pass behind the iris, and it is then of course out of sight, so that its movements can only be guessed at by observing the length and motion of the uncovered portion. Hence injurious friction of the uveal surface may be set up, or, on the other hand, an insufficient opening may be made in the capsule, either of which errors may lead to subsequent disasters. The use of Wecker's cystitome forceps only renders the danger still greater.

Contrasted with these methods let us consider the plan of opening the capsule by means of a curved cataract-needle, introduced through the cornea before the corneal section has been made. This operation was advocated by M. Correnti, of Florence, in 1872. He thinks the chief advantage is in the infiltration of the aqueous humour between the capsule and the

lens, and that this facilitates the subsequent escape of the cataract; and he was very well satisfied with the result in several operations by Graefe's plan, in which he had tried this preliminary laceration of the capsule (see *Annales d'Oculistique* for September-October, 1872.)

Having operated in this way in seven cases with satisfactory results, I have come to the conclusion that it offers the following advantages in practice:—

1. The pupil, if previously dilated by atropine, remains dilated, and the iris, therefore, is out of the way of the needle.

2. The pupillary area is clearly seen, and the movements of the needle, therefore, can be guided into the precise positions required.

3. The nature of the cataract is more clearly ascertained than can be done by means of focal illumination or ophthalmoscopic observation. The density and thickness of the cortex, and its amount relative to the bulk of the nucleus, can be more satisfactorily made out.

4. There is no danger of dislocating the lens or of rupturing the suspensory ligament and so leading to almost certain loss of vitreous.

5. The information derived from the appearance of the cataract, and especially of its cortex, after lacerating the capsule, enables the operator to modify the subsequent steps in accordance with the varying bulk or density of the cataract or its capsule. If, for example, it is found that the nucleus is very large and dense, a large corneal section will have to be made, and an iridectomy also if there is any difficulty about the escape of the lens with moderate pressure. If, on the other hand, the cataract is composed of a bulky cortex with a very small nucleus, a small corneal incision will suffice.

If the capsule be tough and thickened by inflammatory exudation, it may be desirable to extract the cataract within its capsule, and this can be done by modifications of the ordinary methods. With a cataract of the Morgagnian variety, having a fluid milky cortex and small nucleus, it will be better to extract it in its capsule; and this condition could be easily ascertained by the introduction of the needle before making the corneal section. It is not so easy to make the diagnosis of a Morgagnian cataract by the ordinary methods.

The method of operating is as follows:—If the patient be not under the influence of an anæsthetic it will be possible, by fixing the eye in the manner employed in ordinary dissection operations, to dispense with the use of the speculum and fixing forceps during the first step. It is an advantage to do without the fixing forceps if possible, as there is then less likelihood of an escape of aqueous humour on withdrawing the needle.

There is nevertheless an advantage attending the use of the fixing forceps—viz., that the needle can be used with more freedom and precision. The pupil should have been previously fully dilated by atropine. The needle used should be slightly curved near the point, and should be the finest possible in the shank. It is entered very obliquely at the lower and outer quadrant about 1" from the corneal margin. Its point is then pressed against the centre of the capsule, and a puncture made with a view to ascertain the consistency and nature of the cortex. This done, the operator decides whether to remove the cataract in its capsule or not. If he decides to lacerate the capsule, the point of the needle is carried round the upper and inner margin of the lens with a semicircular sweep, and then across the lower and inner margin with a second sweep, the two semicircles being joined above and below by separate movements of the needle if necessary. It is then withdrawn slowly and carefully; and if its passage through the corneal layers has been sufficiently oblique, there will be no loss of aqueous humour, and the pupil will remain dilated as before. If, however, aqueous has unavoidably been lost, the operation had better be completed on the following day, or even a week later. The subsequent steps of the operation will depend upon the information obtained in the first step. Any of the numerous modifications of Graefe's operation, or the old flap operation, or the Warlomont or Liebreich operation, may be employed according to the supposed suitability of the particular method to the case. Having, however, satisfied ourselves that the capsule is freely lacerated, it will generally be found that a much smaller corneal section will be necessary than if this has not been previously done; for the separation of the capsule from the cortex, and of this again in some measure from the nucleus, will have been favoured by the admission of the aqueous, and hence a much less amount of pressure will be required, and a smaller opening will be needed for its extraction. The only possible objection that is likely to be urged against this operation is that it may occasionally cause an escape of the aqueous humour, and so delay the completion of the operation; but this is an objection that will not have much weight. It is not likely to happen often, and when it does is of no consequence. In order to avoid it, it is necessary to have a very fine needle and to enter it very obliquely through the corneal layers. Perhaps some may object that the capsule cannot be so freely lacerated in this way, from the limitation of the movements of the needle; but this is a matter of experience, and it is practically not a serious obstacle to the success of the operation. It is essential, however, that the needle should be introduced on the temporal side of the cornea, either in the upper or lower quadrant; and

hence the operator, if standing behind his patient, must use the left hand for the left eye, and *vice versâ*. If not ambidextrous, he had better, on operating upon the left eye, stand facing his patient and use his right, but this is hardly so convenient a position for the subsequent steps of the operation, and does not allow of so good a rest for the hand in this step as if the operator were behind. Each operator will find out for himself the most convenient and easy attitude, but for the ambidextrous there can be no doubt that the position behind the recumbent patient offers many advantages throughout the operation, and especially in the laceration of the capsule.—*Med. Times and Gazette*, May 9, 1874, p. 499.

76.—LACHRYMAL FISTULA CURED BY A SINGLE INJECTION OF TINCTURE OF IODINE.

This case is recorded by Dr. del Toro in the *Cronica Oftalmologica de Cadiz*. The patient, a young woman of twenty-two, had been affected for three years with a fistula of the lachrymal sac of the right eye, which took place after the formation of an abscess. Pure tincture of iodine was injected by means of one of Anel's syringes. Inflammation of the internal membrane of the sac, extending to the conjunctiva, was the result. Fomentations with a solution of borate of soda were applied, and complete cure was effected in the space of four or five days. Dr. Toro mentions that the existence of conjunctivitis confined to one eye ought always to induce the surgeon to suspect either the presence or the imminence of lachrymal fistula.—*Lancet*, June 6, 1874, p. 804.

77.—THE ADMINISTRATION OF CROTON CHLORAL HYDRATE IN INTOLERANCE OF LIGHT.

(Cases under the care of Mr. BADER, at Guy's Hospital.)

Case 1.—Anne B., aged 18, was admitted with corneo-iritis of both eyes and great photophobia. She commenced the croton chloral hydrate on December 24, 1873, taking five grains three times a day. Daturin of the strength of one grain to one ounce of water to be applied six times in one hour every evening. On January 16, 1874, all the intolerance had subsided; and on the 27th she was discharged, having had no return of the photophobia.

Case 2.—Mary C., aged 11, was admitted with pannus with granulations. Commenced croton chloral hydrate grs. v. three times a day on January 16, 1874. Her left pupil was very contracted, and intolerance was so great she could hardly bear to open her eyes. On the 23rd, there being no improvement, the

dose was increased to ten grains. On the 31st the dose was increased to twelve grains four times a day, after which she was slightly better. Slight improvement continued until February 14, when the dose was increased to fifteen grains three times a day. On the 16th she omitted the medicine purposely, and the next day was not so well. Medicine omitted altogether on the 24th, and greenstone applied to lids.

Case 3.—Mary S., aged 19, admitted with corneo-iritis with great photophobia. Commenced croton chloral hydrate on December 24, 1873—dose, grs. v. ter die sum. On January 1, 1874, intolerance had greatly subsided. On the 18th she could look at the light without flinching. On the 23rd all intolerance had subsided, and she was discharged on February 2, without any return of the photophobia.

Case 4.—Florence B., aged 8, admitted with corneo-iritis, epiphora, and photophobia. Commenced croton chloral hydrate gr. v. ter die sum., on January 13, 1874. On the 17th was able to look at the light. Improvement continued until February 6th, when she was not so well on account of the fog, which was very dense. Dose increased to ten grains ter die. On the 16th almost all the intolerance had subsided, so the medicine was discontinued. On the 18th she was not so well, so the medicine, five grains ter die, was again given, and on March 2nd all the intolerance had subsided.

Case 5.—Celia S., aged 24, admitted with closed pupils, ciliary redness, and photophobia, tension—1, the left eye being the worst. Commenced croton chloral hydrate January 13th, 1874, gr. x. ter die. On the 17th there was slight improvement. On the 23rd she could look at the light with both eyes, but the left still continued worse than the right. On the 27th all the intolerance had subsided in the right eye. On February 9 almost all intolerance had subsided, and she went out of the hospital.

Case 6.—Mary D., aged 15, admitted with pannus with granulations and photophobia. Commenced croton chloral hydrate January 21, 1874, gr. x. ter die. On the 23rd photophobia had subsided considerably. On February 9 intolerance had greatly improved; dose increased to fifteen grains ter die. On the 14th there was very little intolerance. On March 2nd medicine omitted, and lids touched with greenstone.

Case 7.—Ellen B., aged 7, admitted with corneitis and excessive photophobia. Commenced croton chloral hydrate gr. viij. ter die, February 5, 1874. On the 9th there was a very marked improvement in the intolerance of light, which continued until the 13th, when the photophobia returned with increased severity. On March 2nd there was no improvement at all. Medicine continued.

Case 8.—Clara S., aged 15, admitted with corneo-iritis and great photophobia, so that she could not bear the slightest light. Commenced croton chloral hydrate gr. x. four times a day on February 10, 1874. On the next day there was a decided improvement in the intolerance. On the 13th she complained of great pain in the head, so the medicine was discontinued. On the 16th she again took five grains ter die, after which the photophobia gradually lessened, and on March 22 there was very slight intolerance.

Case 9.—Mary B., aged 49, admitted December 30, 1873, with closed pupil, intolerance, and neuralgia over forehead. Commenced croton chloral hydrate gr. xv. ter die on February 6, 1874. On the 10th there was no improvement in intolerance, but pain in forehead was less. Increased the dose to twenty grains ter die. Intolerance did not subside, but pain in head grew less.

Case 10.—Julia B., aged 47, admitted February 28th, 1874, with intolerance of light, but no ocular changes. Commenced croton chloral hydrate gr. xv. ter die on March 1. She gradually improved, but photophobia did not entirely subside. Went out March 20.

Remarks.—It appears that only young people, and only those suffering from genuine syphilitic corneo-iritis, were benefited by the croton chloral hydrate. As regards the dose, I found that between five and ten grains was the most beneficial amount, although in one case I gave twenty grains four times a day without producing much good or any bad symptoms. All three patients complained of the nasty taste of the medicine, but in only one did it appear to produce any symptoms which caused me to stop giving it.—*Medical Times and Gazette*, August 8, 1874, p. 145.

78.—A PROTECTIVE BANDAGE FOR THE EYE.

By F. BULLER, Esq., House-Surgeon, Royal London Ophthalmic Hospital.

In cases of gonorrhœal ophthalmia of one eye only, the first and most urgent duty of the medical attendant is to protect the other from contagion. For this purpose various devices have been proposed, but most of them, if not all, have been defective in one important particular—namely, the eye to be protected had also to be excluded; so that the unhappy patient, in addition to his other troubles, was condemned to grope in darkness throughout the duration of his distressing complaint. Under such circumstances very few persons will keep their hands away from the obnoxious appliance, and as a

consequence the very means intended to secure safety become a source of danger.

[The following is a description of Mr. Buller's bandage.]

It consists of a square piece of macintosh into the centre of which a watch-glass is fastened, and of three strips of adhesive plaster. The macintosh is trimmed to fit the nose and forehead of the patient, and should extend across one side of the forehead about half an inch above the eyebrow, and downwards nearly to the tip of the nose, the nasal portion reaching a little beyond the median line.

The adhesive plaster (*emplastrum resinæ* of the *Pharmacopœia* answers very well) is *spread thickly* on doe-skin; and a strip, about an inch in width, and long enough to reach from just in front of one ear to a corresponding point on the opposite side, is applied along the upper border of the shield. The second strip may vary in width according to the height of the nose, and must be snipped in three or four places, in order that it may be adapted to the uneven surface upon which it rests, the lower part only slightly overlapping the edge of the shield. For additional security, a third and somewhat shorter strip is placed along the dorsum of the nose. The eye is thus completely protected by a waterproof shield, the upper and inner sides of which are firmly adherent to the skin of the forehead and nose, whilst the lower and outer borders are free, so that the eye is exposed to the air almost as freely as when an ordinary shade is worn. Moreover, the surfaces of the watch-glass being parallel, vision is not interfered with, and the patient is able to attend to the affected eye.

As the strips of adhesive plaster become softened in the course of a few days by the warmth and secretion of the skin, they require to be renewed. This may be done as often as necessary without any difficulty or danger of infecting the healthy eye. I have used this bandage in gonorrhœal ophthalmia, and in several cases of inoculation for pannus of one cornea, where the other was clear and would almost inevitably have perished in event of becoming infected. In every instance the healthy eye has enjoyed a perfect immunity from the terrible malady of its fellow.

The glass does not become dimmed by an accumulation of moisture from the conjunctiva and skin of the face, as seems to have been the case with Snellen's closely-fitting glass shield; nor does the protection afforded by the apparatus seem to be at all the less perfect in consequence of its outer and lower borders being free.

The shield may be obtained, at a small cost, from Messrs. Weiss and Co.—*Lancet*, May 16, 1874, p. 690.

AFFECTIONS OF THE SKIN, ETC.

79.—CARBOLIC ACID IN THE TREATMENT OF ECZEMA.

By Dr. J. G. THORNLEY.

Case 1.—F. G., a respectable married lady, æt. 48 years, had suffered from a patch of eczema on the inner side of the thigh for upwards of twelve years. On my first seeing the disease in question, it extended over a surface about twice the size of a crown-piece. The irritation and itching at times were intense, and in consequence, the part had been considerably torn and scratched; there was a copious discharge of an acrid, thin, and serous fluid. The fluid was so irritating as to give rise to inflammation of the surrounding skin wherever it came in contact with it. When scratching, the blood flowed freely, and the integument was cracked and fissured, so much so, as almost to constitute that form of the disease termed by the French, *eczema fendillé*, and by Dr. McCall Anderson, "*eczema rimosum*." The irritation and itching caused by this affection had been for years a source of great discomfort to this lady. Many practitioners had been consulted, and topical applications and internal treatment had been tried to a large extent. Amongst other remedies, arsenic, corrosive sublimate, and bark, and various preparations of iodine had been largely employed internally, while externally lotions, and unguents of zinc, alum, chloroform, hydrocyanic acid, and iodine had been used extensively.

The treatment was commenced by administering a purgative, and a bitter tonic to rectify the digestive organs, which were slightly out of order, and a lotion, as follows, was applied two or three times a day, the part having been first cleansed by carbolic acid soap and tepid water, in which was dissolved a little carbonate of soda:—*Rx.* Acidi carbolici, 3 iss.; glycerine, 3 ss.; aquæ, ad 3vj. This lotion was applied by means of a sponge, the carbolic acid soap being used daily, and in less than ten days the improvement was visible, and in two months the affection had entirely disappeared, and since then has not shown the slightest symptom of returning.

Case 2.—This was a case of *eczema rubrum*, occurring in the wife of a labouring man. A considerable portion of surface in this case was attacked, there being several patches of the disease on the trunk and thigh, while the lower extremity, from the knee to the dorsum of the foot inclusive, was entirely covered by it. The vesicles were numerous, and exuded a large quantity of a clear, slightly viscid fluid. The integument on which the vesicles were seated was tumefied, and between the

vesicles was tense, shining, and of a crimson-red colour, altogether presenting a very characteristic form of this affection. There was a considerable amount of pain, heat, and stiffness in the lower extremity, but no constitutional disturbance.

The treatment was commenced by administering a purgative, the leg to be sponged with tepid water, in which was dissolved ʒ ij. of sodæ bicarb. to the Oj. A weak carbolic acid lotion was then applied, but it was found that the lint stuck so tightly to the eczematous patches, owing to the amount of exudation, that the act of pulling it off irritated the skin. An ointment of ungt. zinci, with carbolic acid and chloroform, aa min. xx. to ʒ j. was applied, and the lotion, after a day or two, used as before. Under this treatment the disease gradually progressed until recovery was complete. I have treated several other cases of eczema after the above manner, and found the treatment successful, together with several cases of prurigo and lichen, in which almost the same treatment was pursued, with a like result.—*Medical Press and Circular*, May 20, 1874, p. 423.

80.—SUBCUTANEOUS INJECTION OF CARBOLIC ACID IN ERYSIPELAS.

Dr. Aufrecht of Magdeburg says, that, having last year under his care four cases of erysipelas of the limbs in old persons, in all of which treatment was unsuccessful, he was led to try the effect of carbolic acid. If erysipelas were the result of the entrance of minute organisms into the subcutaneous connective tissue and of their multiplication, and if carbolic acid had the property of destroying such germs, then, he argued, it should arrest the spread of erysipelas. To ascertain that the hypodermic injection of carbolic acid was harmless, he injected into his own subcutaneous tissue six decigrammes (about ten minims) of a one per cent. solution. No local or general disturbance followed. In July of last year, he applied this treatment in the case of a woman aged 56, with erysipelas of the forearm and hand, following a slight abrasion; and in January he used it in the case of a man aged 82, who had erysipelas of the leg after the breaking out of a cicatrised ulcer. In the first case, he injected carbolic acid night and morning for three consecutive days—making five injections in all; in the second, four injections in two days; they were made into the healthy subcutaneous tissue in the neighbourhood of the erysipelatous portion lying nearest to the body. The erysipelas did not spread in the direction of the part where the injection was made; but some isolated patches above the points first injected necessitated the more frequent repetition of the remedy. Not only was the erysipelas arrested, but the

fever and the frequency of the pulse were reduced, and the general condition of the patients was improved. The erysipelatous swelling and redness diminished perceptibly, remaining only two days after the injection.—*British Medical Journal*, April 25, 1874, p. 550.

81.—ON GANGLION.

By LAMBERT H. ORMSBY, Esq., Surgeon to the Meath Hospital, Dublin.

This is an encysted circumscribed swelling connected with the sheaths of the tendons passing in front and round the ankle-joint and other joints in the body. There are two kinds: 1. The circumscribed; 2. Diffused. This last is generally seen in the palm of the hand and in front of the wrist-joint, and frequently contains small bodies resembling melon seeds. The circumscribed is slow in its growth, and varies from the size of a walnut to that of a pigeon's egg. I have seen lately a man at the Meath Hospital, a soldier, æt. about 40, who, I might say, had a ganglionic kind of diathesis, for there was hardly a tendon passing near a joint that was not affected with a ganglion developed in its sheath, and in some cases two and even three on the same tendon. He said they had come on gradually. They are very common on the tendon passing in front of the ankle-joint, above or immediately below the anterior annular ligament; they are occasionally translucent, and contain a clear, glairy, white-of-egg fluid. They are treated in many different ways: on the back of the wrist, the old barbarous way was—1. To burst, or squash them with the back of a book, by striking the summit of the ganglion with great violence and rupturing the sac and allowing the fluid to escape in the areolar tissue and along the course of the tendon, and ultimately be absorbed. 2. Dividing the wall of the sac subcutaneously with a sharp tenotome, and allowing the fluid to escape in the same manner as before mentioned; 3. Cutting into it, and letting out the contents; 4. Injections with tincture of iodine, one part to two parts of water; 5. Pressure with a convenient permanent compress. Firm pressure with the two thumbs for a short time causes them to disappear, but they appear again and become as large as ever in a few days.—*Medical Press and Circular*, April 22, 1874, p. 332.

82.—ON PROVISIONAL BURSÆ, BUNIONS. CORNS, &c.

By LAMBERT H. ORMSBY, Esq., Surgeon to the Meath Hospital, Dublin.

We frequently see in close proximity to joints where undue pressure is applied small bursæ developed, and often attain a

considerable size producing deformity ; this occurs often at the metatarso-phalangeal joint of the great toe, and at its inner side. This enlargement is termed *bunion* ; it is very painful and annoying, preventing the person thus affected the means of locomotion ; the integument over this swelling assumes a dusky red colour, and is considerably increased in size and temperature ; the pain is increased by a continuance of the pressure ; this affection has the effect of causing the transverse diameter of the foot to be greatly augmented. The same enlargement, with bursal development, is found on the outside of the foot, at the metatarso-phalangeal joint of the fifth, or little toe. When this inflammatory process passes away, there often results a permanent enlargement and thickening of the integument over the joint, producing what is termed by chiropodists a callosity, which is exceedingly painful to the touch, or when undue pressure is applied. Closely allied to these bursæ are what are termed corns, which are so common on the toes of some people, and particularly on those who are in the habit of wearing tight boots. They seem to be nothing more nor less than a thickened state of the cuticle, and may also occur between the toes, when they are called soft corns, and also they are found on the sole of the foot, where they are at times most painful. We often find a small cavity at the base of these callosities when we pare them down deep enough, and the base, by constant picking, cutting, and paring, the corn becomes inflamed, and sometimes suppurates.

The treatment for these two affections agrees very much in at once removing the tight boots and undue pressure, and soft pliable leather shoes recommended, applying simple cold-water dressings to the painful bunion ; in the inflammatory stage, if suppuration occurs in its cavity, it should be cut into by early incision. To prevent undue pressure various methods have been suggested from time to time—a round piece of leather or condensed piece of wool having an adhesive side next the skin, and a round hole cut out of its centre to receive the corn or bunion, and this then applied round the periphery of the swelling ; this, no doubt, is a very useful plan, but act, as it may do, most efficaciously for a time, it is only palliative, and does not effect a permanent cure. The topical application of nitrate of silver produces a hardened black portion of epidermis over its summit ; this, after a time, gets detached, and you can peel it off, and when this is removed touch it again with the nitrate of silver, and so on until all traces of the callosity disappear. I have over and over again tried this plan, and it has seldom failed in my hands, but it must be persisted in. Chiropodists, a class of people who think themselves very clever, profess to take a “ corn out by the roots ”—a most unscientific observa-

tion, to say the least of it—and in reality all they do is, by a patient and gentle paring process, remove a great part of the thickened cuticle, and recommend a well-fitting boot, and simply by removing the cause it often effects a cure. You must also bear in mind that a very loose boot is nearly as bad as a very tight one. Glacial acetic acid is also recommended to be applied after the callosity has been well softened by keeping the feet in warm water for a short time. When the callosity occurs in the sole of the foot and a circumscribed enlargement is seen, if directions are given to a bootmaker, he can make an allowance in the sole and form a slight depression, so as to prevent undue pressure to the foot; in fact, boots of any dimensions and shape can be made by an intelligent bootmaker to suit any deformity occurring in the toes or feet. Well-made boots prevent much annoyance and deformity, while badly-made and ill-adapted boots are a very frequent cause of deformity, corns, bunions, limping, &c. Another distortion often seen is what is called *Hammer-Toe*; it is caused generally by wearing boots of insufficient length, and the toes are bent up and considerably flexed to accommodate themselves to their cramped position; after a time the flexor tendons become permanently contracted, and the toes assume a form not unlike the appearance of a hammer—hence the name; the great, second, and last toes are most often affected.

Treatment.—To recommend longer boots, and allow the toes space to travel forwards. Tenotomy of the contracted flexor tendons may be resorted to, and a spring-pad attached to a sole of leather or other light material, and the toes bent forcibly down and bandaged to this sole, which may have slits in it corresponding to the interdigital spaces, which facilitate the bandage passing between the toes and keep them permanently extended.

Gouty and rheumatic enlargements of the toes are very often seen in persons affected with these diseases. It frequently produces considerable and unsightly deformity at the joints of the toes, most painful at times, and when the patient has this gouty diathesis an attack comes on suddenly, termed a “fit of the gout,” and often affecting the feet—hence the old name given to the disease—*podagra*, meaning a seizure of the feet. These concretions have a chalky-like appearance, are termed *tophi*, occasionally called, although improperly, “chalk-stones,” and are principally composed of urate of soda. These enlargements may remain inactive for a long period, or may become inflamed and ulcerate and expose these calcareous concretions, which often crumble away in a thin puriform discharge. The joint they are deposited round is completely destroyed by gradual structural change, and all motion is lost, and even in

a very slight deposit stiffness of the joint is nearly always the result. As it is apparent that such concretions are merely the effect of a constitutional or blood disease, your treatment should be directed to the disease itself, and to endeavour if possible to correct the poison, whatever it may be, whether uric or lactic acid, by proper remedies, and prevent the attacks by remedies so highly vaunted in the rheumatic and gouty diathesis, such as some of the preparations of potash, lithia, colchicum, guaiacum, &c.—*Medical Press and Circular*, April 21, 1874, p. 331.

83.—ON THE TREATMENT OF EXOPHTHALMIC GOITRE WITH BELLADONNA.

By Dr. R. T. SMITH, Physician to the St. Pancras Dispensary.

[Cases of exophthalmic goitre are most distressing, both from their nature and the difficulty in relieving them. The protruding eyeballs, the wild anxiety of countenance, and the over-action of the heart, form a tableau once seen not readily forgotten. The disease was first clearly described by Graves, of Dublin. We omit the first part of the first case described by Dr. Smith, including the remedies which failed, and pass on to that which succeeded.]

In July, five months after first seeing her, I saw her again, and found her worse than ever. Pulse 140; anæmia most profound; the eyeballs appearing as if they would burst from their sockets. She was now confined to bed by extreme weakness, and by the painful palpitation which occurred on the slightest exertion; but the symptom most complained of was the burning heat which came over her from time to time, and terminated in a drenching perspiration, leaving her in a most exhausted condition.

Aconite was next given in doses of one minim every hour, but was withdrawn after two days' use, as it seemed to give no relief. I was now induced to try belladonna, as a mere experiment, on two grounds: first, that the disease is considered by some to be essentially a paralysis of the cervical sympathetic; secondly, the statements of Dr. John Harley and Dr. Meryon, that belladonna is a stimulant of the sympathetic. Five minims of the tincture were given every hour. Compared with previous treatment the effect was surprising. In two days the pulse was 90, the palpitation very materially relieved, and the outbursts of perspiration very much subdued. In four days the pulse was 80, and on the fifth day the patient walked from King's Cross to Paddington and back again. For ten days no other drug was given, and it had once to be suspended owing to an intercurrent attack of diarrhoea. The dose was then

reduced to fifteen minims four times a day ; subsequently iron was added.

The relief of the palpitation, of the quick, throbbing pulse, and of the profuse perspiring was, so to speak, immediate. The patient was restored to comfort and ease in a fortnight. Amelioration in other respects was gradual, and is still progressing. The diplopia was removed in six weeks. It was quite two months before decided improvement in the exophthalmos could be reported.

Now (May, 1874), no sclerotic is seen above the cornea when the eye is wide open ; the enlargement of the thyroid is nearly gone, and the patient is now in a very fair condition of health and strength.

Case 2.—Miss H., aged twenty-six, came under my care in May, 1873. For several months she had noticed that the neck had been enlarging on the right side, this condition being accompanied with beating in the neck, palpitation of the heart, and great exhaustion. She was very restless and fidgety, and refused to see her friends on account of the scarlet suffusion of the face which came over her on the slightest excitement, together with the beating in the neck and the palpitation of the heart. There was slight anæmia, but no amenorrhœa. Pulse 130. The thyroid gland was greatly enlarged on the right side, and pulsated, with a marked systolic thrill. In this instance there was no exophthalmos, but the eyelids were swollen, lachrymation was often profuse, and the patient complained very much of the feeling of grittiness in the eyes. There was no conjunctivitis. Having known the patient previously, one could not help noticing the change in the facial expression. The face was pinched, the alæ nasi depressed, the features had become quite coarse, and the skin had assumed a brown, dull, unwashed appearance. There was no cough, no pyrexia, and, as in the other case, no dyspnoea. Constipation was a most troublesome and obstinate symptom. She suffered very severely from attacks of “flushings,” which occurred most frequently at night, and on account of which she would have the windows thrown open. These “beats” were accompanied by violent palpitation of the heart, and terminated in profuse perspiration, and often in great prostration.

Treatment.—For three months various remedies were tried, in the following order : iron ; digitalis ; valerianate of zinc ; ice to the neck ; aconite ; iodine internally ; and the constant current : with very little if any relief. In July I began the use of belladonna, giving five minims of the tincture every hour. The effect in relieving the palpitation, reducing the frequency of the pulse, and in subduing the “flushings” and perspirations, was quite as manifest as before ; but meanwhile other

symptoms had appeared—namely, severe headache, occasionally followed and relieved by copious epistaxis, and attacks of angina, which were on two or three occasions so severe as to require the administration of chloroform. Belladonna alone quickly relieved all these symptoms, and in a month's time she might fairly be considered cured. The muddiness of the complexion was gone, and her natural expression was restored; the pulse was 70, and the palpitation removed. But the thyroid gland has undergone little reduction, and has become firm and hard by a fibroid change.

It is a rather interesting fact that in both patients there was a considerable development of fat after the relief given by belladonna. In both cases there have been some recurrences of the symptoms, but they have speedily given way on resuming the treatment. The drug was not given during sleep, and it is noteworthy that reduced doses sufficed as the treatment was continued. Harley states that moderate doses cause contraction, large doses dilatation of the arteries; and it is therefore of great importance that the varying susceptibility of individuals in reference to this drug be kept in mind.

But as there are cases of palpitation quite unassociated with exophthalmos and enlargement of the thyroid, which are greatly benefited by belladonna, it is possible that in the above two cases the relief given was primarily through the heart, the drug acting sedatively thereon.—*Lancet*, June 27, 1874, p. 902.

84.—THE TREATMENT OF VARICOSE ULCER BY LOCAL DEPLETION.

By CHARLES W. HAMILTON, Esq., Medical Officer of
Louisburgh Dispensary District.

Mr. D., æt. 50, a Scotch steward, of healthy appearance, consulted me about his leg in December last (a lady of rank in this country having recommended him to place himself under my care). The history of the case was as follows: The entire leg was in an exceedingly varicose condition, varicose pouches occupying the entire anterior surface of leg and thigh; an ulcer the size of a shilling was on the skin, the centre of which touched the anterior part of the tibia. When I saw him the entire leg was exceedingly inflamed, with a deep-seated and severe pain about the seat of ulcer; the latter was of an indolent type, with a hard edge and excavated centre, touching the bone, the latter symptoms being referrible to his active habits of life. The ulcer he had had for a period of nine or ten years, and during all that period, it never had healed. He was treated in the usual way—viz., the sulphate of magnesia in

mixture, with rest, which soon removed the painful symptoms, and the use of the bandage soon reduced the limb to its normal size. The treatment was now directed to the broken surface, the hard edge of which was removed in due time by lunar caustic, and the parts about the ulcer were daily strapped, after Banyton's method. He had been now five weeks under treatment; the ulcer had got smaller and the edge had disappeared; still, it showed no sign of healing. The sixth week of treatment showed the surface of ulcer still obstinate to cure (every precaution with regard to tonics, bowels, bandaging, &c., being strictly observed). The patient now considered himself incurable, and with some trouble was persuaded to lay up for a short time longer. The prospect of failing, after so long a course of treatment, was anything but pleasant. I now clearly perceived something must be retarding the progress of the cure. A considerable amount of redness remained around ulcer, the capillaries being in a state of chronic congestion. With the view of relieving this congestion I removed with a lancet not more than a drachm of blood, dressing it with carbolic oil, and strapping. On my next visit, which was in two days following, I found the appearance of the ulcer very much improved, and showing every symptom of healing; accordingly, I repeated the operation, and on my next visit, when I removed the strapping, *no ulcer could be seen*. I now procured from Messrs. Fannin a legging and knee-cap, which he constantly wears, and is now quite well, and his leg gives him no trouble.

Thus, by the "*local*" abstraction of a few drops of blood was this old ulcer, so obstinate to any other kind of treatment, cured.

The above case, I think, shows the advantage of *local depletion*, which may be applied with benefit about any broken surface where chronic congestion of capillaries appears.—*Medical Press and Circular*, April 29, 1874, p. 352.

85.—ON THE DIAGNOSIS AND TREATMENT OF PSORIASIS,

By Dr. DYCE DUCKWORTH, Assistant-Physician to St. Bartholomew's Hospital.

The ordinary form of psoriasis is usually seen in persons who are robust, and it is very common to find that the disorder has recurred several times. It is seldom met with in children under six years of age. The sexes appear to be about equally liable.

It is not often that one can elicit a distinct history of gout in cases of psoriasis; but, as an indication of a tendency to such a diathesis or of some condition allied to it, it is frequently the case that lithiasis occurs in these patients. I am inclined

to believe that a form of eczema rather than psoriasis is the cutaneous manifestation of a gouty habit, and its characters are the following:—The patches may be variously situated, not unfrequently being found upon the face and lower extremities; they tend to assume a dry condition without much scalliness; they are intolerably itchy and stinging, and in colour they are dark crimson. One peculiarity is that they are apt to recur upon the same locality; another is especially noteworthy—that while they are not amenable to arsenical medication, they sometimes speedily yield to so-called antiarthritic remedies, such as alkalies and salines. Such dry eczematous patches should hardly be confounded with the paler, more scaly and diffused ones of true psoriasis. There can, however, be no doubt that in some persons attacks of psoriasis alternate with those of articular gout.

In the majority of cases we have to confess our ignorance as to the underlying condition of which this disfiguring affection is significant. Some writers maintain that it is in some way related to latent syphilitic taint. This view is not, however, borne out by the results of treatment. Again, the alleged frequency of the disorder amongst the Jews has led some to conceive that this squamous disorder is a relic of leprosy. This view cannot be held to be proven in any way. It is in some respects satisfactory to know that vigorous and healthy persons are most commonly the sufferers. The disease certainly runs in some families, and hereditary tendency to it exists.

Treatment.—In such cases as are manifestly due to syphilis there is no difficulty in applying proper remedies. Small doses of perchloride of mercury, with or without iodide of potassium, are most useful. Sometimes iodide of potassium alone is sufficient to remove the disease, and the doses must be gradually increased according to the effect produced. Locally there is much to be secured by the employment of such mercurial preparations as irritate least, and the best of these are the five per cent. solution of oleate, or a fifteen grain to the ounce solution of bycyanide of mercury. Calomel or vermilion fumigations are also of marked service, two or three of these usually proving sufficient. Salivation does not follow this practice, unless the patient inhales the vapour of the volatilised salt.

Recurrence of the disorder is apt to follow if the remedies be too soon withheld, and in every case the patient should remain under occasional observation for two or three years. Attention to the general health is of much importance, regular habits and strict temperance being especially enjoined.

In the management of true psoriasis there is scope for much discrimination. As in all cases of disease, the patient, and not his malady, should be treated. In most instances there is much

to be done locally. I am convinced that the favourite dictum of Professor Hughes Bennett, of Edinburgh, with regard to this disorder, has much truth in it. He used to teach that pitch externally, and arsenic given internally, would cure any case of psoriasis in six weeks, and I have frequently witnessed the good results of this treatment in his hospital practice. It must, however, be stated that during this period the patient either kept to his bed or lived in clothes constantly smeared with pitch ointment, and I have little doubt that much of the success of the plan depends upon the *constant* application of the local remedy. Hence this method is only suitable, as a rule, for hospital patients. Frequent baths also formed part of the Edinburgh treatment. The arsenic was given in the form of Fowler's solution together with half-drachm doses of liquor potassæ.

It must be admitted that this plan of medication is not so universally applicable as the above-quoted dictum might lead one to believe. Many cases of psoriasis are intolerant of the action of pitch. In certain persons, and in particular stages of the disorder, the remedy proves too irritant; and, again, arsenic, even in the smallest doses, cannot be borne by some patients. Hence, in many instances, the employment of pitch has to be carried out in a modified form. I have a high opinion of the value of the application in the majority of cases. If pitch ointment is objected to, some of the tarry oils, or carbolic acid, may be substituted; and the odour may in some degree be covered, as Dr. McCall Anderson has shown, by the oil of lemon-grass (*citronella*) or by rosemary. The tarry preparations, and all local remedies indeed, should only be applied to the affected spots, and with as diligent friction as can be borne. The linimentum picis comp. is a favourite application at St. Bartholomew's Hospital; and is composed of equal parts of pitch, soft soap, and rectified spirit of wine. It is best laid on with a painter's brush, or with a piece of flannel.

Warm alkaline baths are of much use, not less frequently than twice in the week; and, if the patches will bear it, they may be effectually cleansed and prepared for fresh inunction by friction with soft soap and pumice-stone.

It will be found, however, in many cases, that this local treatment is resented, and this is to be looked for in the early stages of the disorder, when the patches are small and hyperæmic, and when fresh spots are still appearing. Irritant treatment is inapplicable to this condition, and, instead, it is proper to employ simple inunction with olive oil, almond oil, or spermaceti ointment. Warm rain-water baths and a lather of warm gruel or decoction of starch are also preferable to alkaline baths.

Later on, good Castile or petroleum soaps may be used to cleanse the patches and get rid of scales. If cracks appear in the lines of motion, and the affected parts become very sore and sensitive, water-dressing is of much service in allaying the discomfort, and it may be practised after anointing with bland oils. A creamy paste of oxide of zinc with a little glycerine-and-water or lime-water may also be used with benefit. The cracks may be treated at night with the glycerine of starch. In the later stages of most cases the tarry applications may be employed. If it were possible to secure a constant contact of pitch, the results would, I believe, be more satisfactory than they usually are. It often happens that patients consent to use this application only at night, and hence a longer time is required to remove the disease.

In the Dublin schools the use of calomel ointment is common. I have tried this preparation in a number of cases, but I think it is less efficacious than pitch. Salivation never follows the external employment of calomel. Nitrate-of-mercury ointment is a more powerful application, and sometimes proves very useful. The oleate of mercury is, however, preferable in many respects. I have known beneficial results to follow the use of two or three calomel fumigations (fifteen grains at a time), even in cases where there was no reason to suspect any syphilitic taint.

Few remedies exceed in value the employment of lemon-juice locally in obstinate cases of palmar psoriasis. The part should be rubbed with a piece of freshly-cut lemon several times a day. It is often difficult to prove the exact nature of chronic palmar psoriasis. Many cases are doubtless of syphilitic nature, but many are quite uninfluenced by anti-syphilitic treatment. Lemon-juice may be tried in all chronic and unyielding cases.

In most cases of chronic psoriasis it becomes necessary, and it is often desirable, to change the local plan of treatment. Thus, mercurial preparations may be used alternately with those of tar. Any remedy that is manifestly beneficial should be steadily persevered with till the disease disappears. Constitutional treatment should be continued for some time after the removal of the patches, and I now pass on to discuss the best methods of internal medication.

There appears to be much unanimity amongst writers upon skin diseases as to the value of arsenical and alkaline remedies in cases of psoriasis. It may, I think, be safely affirmed that the majority of cases are benefited by arsenic. It is certainly more useful when the acute stage has passed away, and when the patches have become very scaly. If it can be borne at all, it can generally be taken in considerable doses. It is best given in the form of Fowler's solution with liquor potassæ or bicar-

bonate of soda in any suitable infusion. Sometimes it is better borne in the form of the Asiatic pill (with black pepper). From three to six minims of the liquor arsenicalis may be given three times a day, and each week the dose may be increased by a minim or two. Mr. Hutchinson's plan of withholding the medicine altogether for a day in each week is, I think, worth following during an arsenical course. I do not lay much stress upon the exhibition of the drug immediately after food, having observed, as many practitioners must have done, that, if arsenic is borne well, it can be taken almost at any time. As there are exceptional cases, however, it is perhaps a safe rule to enjoin upon most patients, and by some persons arsenic can only be taken after food. The dose may be carried to the extent of ten or fifteen minims if need be, and the larger quantities will often accomplish results which are quite unattainable by the smaller. I believe many cases of psoriasis are allowed to linger on unbenefited because arsenical medication is not sufficiently and boldly pushed. If there is clearly an intolerance of the remedy, it is proper to prescribe simple alkalies, and an effort may be made to act more freely upon the kidneys. Thus, nitrate of potash, acetate of potash, and nitrous ether may be given. Pitch, in the form of capsules, to the number of three or six in the day, each containing five minims, may be tried; likewise carbolic acid, in doses of one or two minims, in the form of pill. With these latter remedies may be classed the balsam or resin of copaiba, and the tincture of cantharides, which are held in repute for the treatment of psoriasis. It is not unlikely that these medicines act beneficially by stimulating the action of the kidneys, and thus relieving the skin.

If there is manifestly a gouty predisposition, it is important to employ alkaline remedies, and colchicum may be given with these; and in these cases it is necessary to employ mercurial purgatives occasionally, and to promote more free and watery evacuations from the liver and bowels by means of sulphates of magnesia and soda or by the action of Rochelle salts.

In most cases of psoriasis, but especially if there is gouty taint, care must be exercised as to proper diet and regiminal habits. Rich and fatty food, highly-seasoned dishes, and undue amount of animal food must be avoided, likewise cheese, pastry, wines, and malt liquors. A plain diet, consisting of fish, fowl, and roast meat, with abundant green food—cruciferous mainly—and free use of water-drinking, is best. Light claret or well-diluted alcohol may be permitted to that large class of patients who will not be persuaded to abstain, even temporarily, from the use of all liquors.

Resort to some of the spas thus becomes a valuable adjunct to most therapeutical measures in the treatment of psoriasis.

The discipline enforced, the facility for bathing and water-drinking, and the avoidance of improper dietetic habits, are all of value to such patients. I believe the sulphurous waters of Harrogate are amongst the best in this country for sufferers from psoriasis. No benefit can, however, be looked for in chronic cases unless a prolonged course of treatment is carried out. It is well worth while for any patient severely affected to try the effects of three or four months of suitable hydropathic treatment.

On the continent the baths of Aix-la-Chapelle, Schlangenbad, and of Leuk, are amongst the best. In the more recent cases the latter are indicated rather than the former. The most suitable waters for drinking are those of Vals, Carlsbad, Freidrichshall, and Püllna.

It will be observed in some cases that the patches commence to clear up from the centre, and a raised marginate eruption remains. The latter must be vigorously treated, and the healing centres may be less carefully attended to. The explanation of the occurrence is probably to be found in the fact that the parts first attacked tend to recover first, while those which are affected later—the extending areas—are the last to be restored to health. Cases met with in this healing stage, constituting one of the annular or *lepra* varieties of the disorder, must be carefully distinguished from gyrate patches of syphilitic squamous affection. In such chronic cases as are termed *inveterate* I should counsel prolonged hydropathic treatment, with careful attention to the weight and general nutrition of the patient.—*Lancet*, July 4, 1874, p. 7.

86.—A METHOD OF TREATING TINEA TONSURANS.

By EDGAR A. BROWNE, Esq., Surgeon to the Eye and Ear Infirmary, and the Dispensary for Skin Diseases, Liverpool.

[We should never have a parasitic disease if the soil on which it grows were not a suitable one. Theoretically therefore we should attain the most complete success by improving the constitutional vigour of the patient, but in actual practice we are frequently obliged to content ourselves with an imperfect fulfilment of this condition.]

Parasitocides are generally applied in the form of lotions, dabbed vigorously into the patches, and kept in contact by soaked pads of lint covered with oiled silk, &c. The objections to the plan are, the frequency with which the application must be renewed, annoying both to patient and attendant, the difficulty (or say, impossibility) of making nurses understand the importance of maintaining a constant and not an intermit-

tent atmosphere over the part, and the trouble of keeping the pads in position, on dependent portions of the scalp such as the occipital region.

In a certain number of cases where one or other of these drawbacks becomes prominent, the following plan will be found useful. A margin of healthy hair is to be cut quite short or shaved round the patch. A brisk rubbing with the *ol. piciss rect.* or some similar hydrocarbon is the next stage, and the reddened and saturated patch is to be thickly dusted with a powder composed of tannin, iodine, and gum arabic. This is to be moistened with a few drops of the oil, and gently but firmly pressed into the skin with the end of a small cork. Repeat the process till the whole patch is covered with a layer of paste about an eighth of an inch thick, and then allow it to dry. The firm hard scab thus formed may be left undisturbed for three or four days, when it should be moistened, scraped off, and reapplied. No home dressing is required in the intervals.

This mode of treatment is not easily applied to patches of more than about an inch in diameter, as the artificial scab is apt to crack and fall off in pieces, nor is it at first received with much favour by parents of the upper classes on account of its somewhat suggestive appearance. Simple as it is, parents and nurses can seldom be trusted to make the application,—the surgeon should act as dresser.

The advantages of the method are the long interval between the applications contrasted with what can be had with any evaporable dressing in common use; the absence of irritation; the prevention of auto-inoculation by combing, &c., owing to the solid roof formed over the fungus; and the avoidance of bandages, pads, impervious night-caps, or other easily disarranged contrivances.

It is at least as rapidly effectual as any other plan with which I am acquainted.—*Practitioner*, May 1874, p. 327.

87.—CASES OF MALIGNANT ONYCHIA TREATED WITH NITRATE OF LEAD.

Under the care of FAIRLIE CLARKE, Esq., at the Charing Cross Hospital.

Three cases of onychia maligna have lately presented themselves at the above hospital, which have all been treated with nitrate of lead, according to the suggestion of Mr. MacCormack.

The first patient was a traveller, aged forty-one, who had malignant onychia of the right little toe. The disease was of ten or eleven weeks' standing. The toe was much enlarged

and clubbed, the nail was almost entirely gone, and there was an ulcerated surface the size of a threepenny-piece. There was severe pain, not merely in the seat of the disease but also in the ankle; and as the patient was obliged by his occupation to be constantly on his feet, it was a very serious inconvenience to him. He came to the hospital on Dec. 12th, 1873. The nitrate of lead was dusted on the ulcer, and he was told to apply it in a similar way every night and morning. He abstained from work for a week, and rested at home, so that he gave himself every chance. On the 19th the following note was made:—"The toe is easier than it has been for a long time past. There is now a hard dry scab on the spot which was before raw and ulcerated." The patient continued to attend the hospital for a week or two longer, the onychia meanwhile slowly improving, and then he was lost sight of. After the lapse of four months he was met walking briskly along the street, following his ordinary occupation. He said that the nitrate had "effected a perfect cure." He had used nothing else, except "a lotion of herbs," and he had got speedily well.

The second case was that of a pale, flabby little boy, aged three years and a half, who was brought on Jan. 20th, 1874, with malignant onychia of the right thumb. The disease began a month before he came to the hospital, and took its origin from jamming the thumb in a cab door. The thumb had a very characteristic appearance, being enlarged through its whole extent, and clubbed at the point. The nail was ragged and misshapen, and the ulcerated surface around it bled freely. The patient was ordered steel wine, and nitrate of lead was dusted upon the sore night and morning. It was said at first that the application gave great pain, so that the child screamed violently; and probably it was not applied regularly. Still the ulceration improved. At a subsequent visit the mother said that the application was less painful, and she asserted that it was regularly used. The case was steadily improving during the three or four weeks that he attended, but afterwards the child was attacked by the measles, and did not visit the hospital. The nitrate was, however, beneficial in this case, and would doubtless have effected a cure if it had had a fair chance.

The third case was that of a married woman, aged thirty-seven, who applied at the hospital on Feb. 3rd, 1874. She was suckling at the time, and was rather weak. The malignant onychia had originated in the prick of a hair-pin two months before. When first seen the right thumb was red, swollen, and clubbed. There was a large ragged ulceration; the nail had perished; and the last phalanx was necrosed, and

was separating. She was ordered to take quinine mixture, and to apply the nitrate of lead twice a day. It gave her very trifling pain, and seemed to produce an immediate amendment. In a few days the last phalanx came away. As there was a good deal of pain in the thumb, poultices were applied over the nitrate. She went on steadily improving, without a drawback, and in six weeks the disease was cured, the thumb remaining of course somewhat misshapen in consequence of the loss of the last phalanx.

On the whole, having regard to the severity of onychia maligna, and the great difficulty of treating it successfully by the ordinary lotions or ointments, Mr. Fairlie Clarke is inclined to think that the remedy to which Mr. MacCormac has drawn attention is a very valuable one.—*Lancet*, May 23, 1874, p. 726.

88.—ON THE DISPERSION OF TUMOURS BY PUNCTURE.

By Deputy Inspector-General CAMERON, H.P.

Those familiar with the East are aware that, from time immemorial, the native hakims have been accustomed to attempt to bring about the absorption of the enlargements of liver and spleen, so common in hot malarious countries, by the use of puncture with long, sharp stilets of considerable thickness. Twining, in his work "On the Diseases of Bengal," mentions the practice.

I have never followed it for the purpose of procuring the dispersion of such enlargements, but I have frequently seen those of the liver disappear rapidly after repeated plunges of an ordinary hydrocele trocar when seeking unsuccessfully for suspected abscess; and I may say here, that I never saw in any instance inflammatory or any other bad symptoms produced by such operations, strange as it may appear to those unaccustomed to perform them. But what I wish to draw attention to is, that other enlargements besides those of liver and spleen may be made to disappear by puncture. Nothing is more tedious than those chronic glandular swellings which, in strumous subjects, often in hot countries follow upon trifling causes, such as angry mosquito bites, riding a rough bucking horse, over-exertion, or a strain in cricketing and so forth. I have seen an officer laid up for many months, and ultimately invalided with a large mass of indurated enlarged glands occupying the whole inguinal region, and resisting all the recognised routine of treatment. Accident showed me that deep puncture of such masses with a common lancet held at right angles to the swelling, and pushed down to its bottom, will often cause absorption to set in and proceed rapidly.

The first case in which this occurred to me was one of a mercantile gentleman, disabled by a mass of swollen inguinal glands, hard as a board almost, and resisting all treatment. This patient's loss of time at office was a very serious matter to him, and, influenced by his despairing impatience, I plunged a lancet perpendicularly into the mass as far as it would reach. The point came out tinged with matter, and hard pressure brought up a little cheesy, ill-formed pus, but no discharge whatever followed, and absorption set in and proceeded rapidly.

An extraordinary and suggestive case occurred to me afterwards, which, in my opinion, affords grounds for thinking that puncture might possibly be found to bring about the dispersion of such growths as fibrous tumours of the uterus. Reasoning from the non-supervention of any evil symptoms after repeated and deep puncture of the liver, even with such a clumsy tool as a small hydrocele trocar, I see no ground for fearing to puncture with a small stilet such a fibrous uterine tumour as is often plainly to be felt through the abdominal parietes, and I think puncture through them less likely to be followed by any evil consequences than puncture per vaginam, owing to the perfect exclusion of air. That an analogous operation of the kind can be done safely and successfully the following remarkable case shows.

When superintending surgeon of the Southern Province in Ceylon, one of my assistants requested my advice respecting an infant of a few months old, whose parents were in a great state of anxiety and alarm about a swelling which they had discovered in its abdomen, and on which the usual constitutional and local treatment produced no effect. A fine healthy infant was brought forward, through whose abdominal walls a firm tumour, about the size and shape of a dove's egg, could be felt with ease. It was smooth, movable to some extent, painless on pressure, and seemed to be situated in front of the upper edge of the quadratus lumborum muscle, as far as one could judge, half way from the spine. I told the parents that nothing operative could be attempted with it, and recommended patient perseverance in the treatment already adopted, assuring them that no change was to be feared if left alone. This did not at all satisfy them; they declared the tumour was steadily increasing, and would ultimately kill their child, imploring me from time to time to save it by an operation. To all this I turned a deaf ear, but they worried the gentleman in charge incessantly, and at last he too begged me to do something with the case, no matter what. On seeing the child again I found that the tumour was certainly increasing, for now it was as big as a full-sized pigeon's egg; still I coun-

selling non-interference, assuring the parents that any operation would in all probability prove fatal. To this they replied that they were quite prepared for such a result, and willing to risk it, as death would certainly follow the steadily increasing enlargement of the tumour, so that really they had all the misery of seeing their only child dying by inches, and nothing done to prevent it! At last, like the importunate widow, they wearied me out, and as in my then position I had not to dread either Mrs. Grundy's remarks or a coroner's inquest, I agreed to "do something," at the same time distinctly stating that inflammation and death would almost certainly be the result of my meddling. Full acquiescence being given, I steadied the tumour between my fingers spread out, and then pushed a lancet, held at right angles, deep into it. The feeling communicated to the hand was that of penetrating a dense glandular structure. No trace of matter appeared on the blade, no diminution of the tumour, nor any sign of internal hemorrhage; so the infant got a suitable opiate and a large poultice over the abdomen, the parents being quite happy at their wishes being granted. It never had the slightest fever or bad symptom of any kind, and, absorption having set in at once, the tumour, whatever it was, disappeared altogether in a very brief space, while I had the praises of my wonderful skill sounded in all directions. With this case and those where a like result followed on puncture of the liver borne in mind, I should be greatly disposed to try a similar treatment sooner than see a patient perish by hemorrhage consequent on fibrous uterine tumour. Those who have never witnessed hepatic explorations are often very slow to believe in their safety and good effects. An old Peninsular P.M.O., new to the East, once saw me push a trocar deep into the liver three times in succession without finding the abscess I was in search of. He stole quietly away, unnoticed, while I was bending over the patient, being fully convinced that death would result. Next morning he met me with a very ominous face, and inquired, "How is your man, sir?" "Oh, very well," I replied. "What, sir! very well! after receiving three deep punctured wounds in his liver?" I advised him to go and see for himself, and great was his surprise on doing so. In that case a perfect recovery took place, the general enlargement wholly disappeared, and the man, a sad drunkard, was soon discharged to duty.

Having, some years ago, been so well abused in the pages of this journal by the Netley Professor of Medicine, for strenuously advocating the early puncture of the liver in every case of suspected abscess, it is with peculiar pleasure that I have read in the Medical Gazette of 25th April last, p. 457, the

brilliant success of the operation at the Madras General Hospital, in a case where two distinct abscesses—one containing four and the other *forty* ounces of pus—were opened in succession in the same patient, instead of leaving him to die of hectic while waiting for “pointing” to take place, as insisted upon by my severe critic. How many lives have I seen sacrificed to such timid practice! My long experience in such cases leaves no doubt on my mind that where hepatic abscess is suspected to exist, not a day should be lost before endeavouring to evacuate it. If this is done while the abscess is still small, and also deep-seated, I am inclined to think that nothing beyond the first emptying of it will be required, and that the canula may safely be withdrawn and the opening closed at once. The last hepatic abscess I opened showed no external indications whatever of its existence. The man had never had any acute symptoms, but was steadily declining in health without any specific tangible cause, so that I was requested to see him in consultation, and diagnosed deep-seated abscess, more by the method of elimination than anything else, save the sort of intuitive conviction which long experience often causes one to feel without being able to say specifically why. I felt certain he had a deep abscess somewhere in his liver, and made two deep unsuccessful explorations for it with an ordinary trocar. Fancying I had not gone deep enough, I took a long rectum trocar, and with that hit the spot, and evacuated about two ounces of thick matter. The usual plan of fastening in the canula was adopted, but it slipped out in the night and could not be reintroduced, in consequence of the resistance and struggles of the man. I expected effusion into the peritoneum and death would result, but to my surprise the man rapidly recovered without the slightest bad symptom, and without any further discharge whatever, thereby considerably enlarging my ideas on the treatment of his formidable ailment; but, unfortunately, I never had another case of it during my subsequent year’s service in India on which to try the improved plan of management with which accident had made me acquainted.—*Lancet*, Aug. 22, 1874, p. 267.

SYPHILITIC DISEASES.

89.—THE SUBCUTANEOUS INJECTION OF MERCURY.

By CHARLES J. CULLINGWORTH, Esq., Surgeon to St. Mary’s Hospital, Manchester.

It was not without reason that Sir William Jenner, in his Address in Medicine at the meeting of the British Medical

Association in 1869, spoke of the introduction of subcutaneous injection into medicine as one of the most important advances of recent times. As with every other valuable invention so with this, it was not the result of a sudden brilliant inspiration, but the crowning point of much careful research and patient experiment. Here, as usual, coarser methods led the way, and, step by step, these were modified and improved ; the object of almost all the earlier investigators being the relief of neuralgia by the direct application of narcotics to the painful part. First of all, in 1824, it was suggested by Lembert and Lesieur to raise a small blister over the painful spot and sprinkle powdered morphia on the denuded surface. Then, in 1836, Lafargue, by means of a lancet puncture, inoculated morphia in its solid form, or made into a paste with water. A little later, morphia in suspension was introduced by means of a grooved needle, or on the point of a lancet, or as was practised by Langenbeck, in a sort of miniature scoop or spoon, partially covered over. This was again improved upon by Rynd of Dublin, who devised an instrument consisting of a needle and canula, the needle being withdrawn by a spring after making the puncture, and a concentrated solution of morphia poured into a little opening in the side of the canula and allowed to run down into the tissues. To Dr. Alexander Wood, however, belongs the credit of raising the new method into popularity by the adaptation of the subcutaneous syringe. He tells us that he had tried first one and then another plan of conveying morphia under the skin, and had found them all unsatisfactory, until, at the end of the year 1853, he had occasion to use one of the little syringes made by Ferguson for injecting solution of the perchloride of iron into a nævus. It struck him that this was the instrument wanted, and he determined to try it in the next case of neuralgia that presented itself. The opportunity soon came, and all the world knows the excellence of the result.

Still it was considered absolutely necessary, in order to produce the desired effect, that the remedy should be injected at the seat of pain until Charles Hunter published a series of careful experiments, and proved this important fact—that the influence of remedies injected subcutaneously is not local, but is exercised through the general system, and that, consequently, the relief will be the same however far away from the affected nerve the narcotic be introduced. After much controversy and inquiry the conclusions of Hunter were admitted to be sound ; and thus the field of experimentation was greatly widened. The subcutaneous method, until now reserved for the administration of anodynes, was evidently capable of more varied use. Wherever, in fact, precise and rapid action of a remedy was required, this was presently seen to be the best way of securing

it. And now we are familiar enough with the injection of quinine in intermittent fevers, ergotin in hemorrhage, Calabar bean in tetanus, certain antidotes in cases of poisoning, and a host of other medicines which are desired to act quickly and powerfully through the system at large.

It with the introduction of mercury into the system by this method that I am now going to deal more particularly. Widely different opinions have been expressed as to the value and even the practicability of giving mercury in this way. It will not be amiss, therefore, to commence with a sketch of the history of this mode of treatment.

The earliest experiment appears to have been made by Chas. Hunter himself, who administered to a girl of twenty-one, two injections weekly of a solution of the bichloride of mercury, containing a grain to the drachm, and continued them weekly until twenty-five grains had been given, without producing salivation. The exmple thus set was followed in the year 1860 by Professor Hebra, and by Neumann, at Hebra's suggestion, in some cases of syphilitic skin disease. They obtained rapid amelioration of symptoms; but, not considering the method preferable to others they soon abandoned it.

In 1864 Dr. Scarenzio, of Pavia, published a memoir entitled "First attempts at the Treatment of Constitutional Syphilis by Subcutaneous Injections." He selected for use calomel prepared in the form of vapour suspended, in his first experiments, in glycerine, and afterwards, in order to expediate absorption, in water alone. He injected twenty centigrammes (three grains) each time in a gramme and a half (twenty-three drops) of the vehicle. For the absorption of the insoluble salt he relied upon the alkaline chlorides of the blood changing it into the soluble bichloride; "for it is certain," he says, "that no physician would risk the injection of a solution of sublimate into the subcutaneous cellular tissue, in the fear of provoking a slough without any chance of absorption." Details are given of eight cases, and in one only was there profuse salivation without benefit. "In all the others there was," he assures us, "a rapid and lasting cure, without accident, either at the time or subsequently." True, every injection that he made was followed by abscess; but he does not rank this as an accident, "for these abscesses were always limited," he says, "to the subcutaneous cellular tissue, and were very quickly cured."

Further trials of this mode of treatment were made by Ricordi, Ambrosoli, and others, at the Hospital for Venereal Diseases at Milan, and in other parts of Italy, where, indeed, it found great favour, and appears to be still in vogue. At a medical congress held at Brussels in 1868 the gold medal was awarded to Drs. Scarenzio and Ricordi for a memoir in which

the calomel plan is recommended very strongly, especially in the syphilis of infants and pregnant women; and on the 4th of January, 1869, Dr. Oscar Max-Van Mons narrated to the Royal Society of Medical and Natural Sciences of Brussels several cases in which he also had used calomel suspended in a few drops of thin mucilage. An abscess the size of an egg invariably followed. "One injection," he says, "often sufficed for a cure; if not, a second was made. . . . one or two days after the opening of the first abscess; . . . a third is rarely necessary." This brilliant and astounding result, however, was not allowed to go long unchallenged. Dr. Aimé Martin forthwith published a letter in the *Gazette des Hôpitaux*, pointing out that it was unreasonable to pronounce cured on the 27th of February cases which had only commenced treatment in November of the previous year. He also condemns the use of calomel on account of its insolubility and the consequent "torture of these abscesses which seem so trifling to M. Van Mons."

On reading an account of Scarenzio's experiments, Mr. Berkeley Hill was led to publish some which he himself had made in 1864-5. He chose for his purpose a solution of corrosive sublimate, on account of its being more readily estimated, and generally injected one-fifth or one-sixth of a grain, in six minims of water, twice a day into the arm or shoulder. His observations extended over a series of eleven cases of constitutional syphilis; in none of which had he abscess, and in only one cellular inflammation, with some little pustules on the skin. The pain of injection was trifling. In nine of the cases the gums were affected after injecting less than one grain and a half of the salt, and in four of these after only four-fifths of a grain.

But by far the most extensive series of experiments yet attempted was that by Dr. George Lewin, of the Charité Hospital, Berlin, who published some observations based on 700 cases—firstly, in the fourteenth volume of the *Annals of the hospital*, and afterwards as a separate work. He used a simple solution of the bichloride in water (about four grains to the ounce), adding a little glycerine and morphia in the case of patients with a sensitive skin. Fifteen drops (one-eighth of a grain were usually injected daily, sometimes in the side of the chest, and sometimes below the shoulder-blade, or on the outer side of the arm. It was exceedingly seldom (only in 2 or 3 per cent. of the cases) that he had to contend with local accidents of any importance—as, for instance, abscesses or sloughing of the overlying skin, and even then he attributes them to the clumsiness of the operator. Fifteen or sixteen injections, involving a total quantity of about two grains of sublimate, were generally

sufficient to cause the symptoms to disappear; and he affirms that the relapses were considerably fewer than after ordinary mercurial treatment. His book concludes with a short statement of the advantages of the method:—1. Rapidity of cure (proportional to the quantity injected daily). 2. Comparative infrequency and lessened gravity of a relapse. 3. Precision as to dose. 4. Facility of administration. 5. Freedom from irritation of the alimentary canal.

After the appearance of Lewin's paper, several inaugural dissertations were published in Germany on this subject, each of them containing a few original observations, and generally maintaining the superiority of the new treatment. Dr. Grünfeld also related fifty cases treated in the wards of Prof. Sigmund of Vienna. In 1405 injections there were only two abscesses. The duration of treatment was much more prolonged than that indicated by Lewin, being, on an average 93·7 days—a difference which the author ascribes to the greater severity of the cases. He made some analyses of the urine, and detected the presence of mercury after the injection of twelve milligrammes (about one-sixth of a grain). “No method,” he says, “is comparable to this in rapidity and in certainty, or lessens so materially the chances of relapse.” He thinks, however, that, owing to the severe pain occasioned by the injection and the liability to abscess, it is chiefly indicated where internal treatment and the use of friction are inapplicable.

Dr. Stöhr, commenting on eighty cases he had observed in the clinical wards at Würzburg, pronounces the subcutaneous treatment to be “the most energetic and direct” mode of mercurial treatment which has yet been proposed. But he, also, characterises the pain of the operation as very severe; and he describes, moreover, a number of very unpleasant constitutional effects—as, for instance, diarrhoea, vomiting, dyspnoea, shivering, feverishness, and even syncope.

The method advocated by Lewin has been adopted by many other observers. I may mention particularly Dr. T. J. Walker, of Peterborough, who published in the year 1869 an account of ten cases; Dr. R. W. Taylor, of New York, who conducted observations on about fifty patients for a period of eighteen months; Dr. McCall Anderson, of Glasgow; Dr. Oppert, of London, who used a solution of double strength, and related to the Clinical Society of London eight cases so treated; Prof. Sigmund, of Vienna, who writes in 1872 that he has applied it in more than 200 cases, and has met with abscess only five times; Dr. Hammer, of the United States of America; Dr. Hansen, of Revel; and Dr. Hagens, a German army surgeon, who records forty-four cases, twenty-two in the military hospital, and an equal number in private practice. All these

observers speak most favourably of the therapeutic results of the treatment. Drs. Taylor and Oppert hesitate to recommend its general adoption on account of the local mischief; while, on the other hand, Hagens says that abscess never occurs except from faulty solution or superficial injection.

At the suggestion of Lewin, M. Liégeois introduced the treatment into France in 1867. He succeeded in avoiding all inflammatory action by using much smaller doses ($\frac{1}{32}$ gr.), and combining with these a small quantity of morphia. On the 2nd of June, 1869, he read a communication before the Surgical Society of Paris, based on researches extending over a period of eighteen months, and including more than 400 observations. He injected fifteen drops in each arm every morning, and found the pain easily tolerated and followed by no inflammatory action. Of 196 cases treated for secondary symptoms, 127 were cured, and the remaining 69 bore no trace of the secondary manifestations for which they were admitted. Six months after cure M. Liégeois was able to say that only four of the cases marked cured had suffered any relapse. He noticed that where the symptoms yielded quickly relapses were more frequent. The average length of time under treatment was from twenty-five to thirty-four days.

[The following is a brief outline of the plan proposed by Dr. Staub, of Strasbourg, in 1872.]

Liégeois had already succeeded in avoiding local accidents by diminishing the dose. The investigations of Staub had for their object the discovery of some form of solution which, while ensuring an equal immunity, would yet permit the employment of ordinary doses, and so lessen the duration of treatment. There are, he says, two principal causes of irritation attending the subcutaneous injection of the sublimate: (1) the known acidity of the solution, and (2) the property it possesses of coagulating the albumen of the tissues. To obviate these inconveniences he proposes to adopt a modification of Mialhe's "normal solution of mercury," which is said to be a solution of the sublimate in that form of combination into which all mercurials enter before they act on the system. It consists in adding to a simple solution of the bichloride in water small quantities of the chlorides of ammonium and sodium and the white of an egg. This solution, Dr. Staub says, is free from acidity, and will not coagulate albumen. It will, therefore, pass directly into the circulation without the formation of a coagulum at the seat of injection; and the delay and irritation due to the process of re-dissolving will be avoided. Forty-four cases of constitutional syphilis are appended, in forty-two of which a cure was effected very rapidly. The average number of injections given to each

patient was thirty-four, and the total quantity injected a little under two grains and a half. The pain of the operation he reports to have been trifling, and only occasionally was induration produced. In one patient, the subject of chronic alcoholism, there was some sloughing. Salivation occurred only five times, the gums were affected four times, and there was mild stomatitis three times. The author claims for his solution that it will not give rise to any irritation at the seat of puncture; neither inflammation, nor abscess, nor eschar. Subcutaneous indurations, Dr. Staub says, are very rare, and become absorbed in three or four days. He concludes by expressing his belief that this method is destined to take front rank in the therapeutics of syphilis.

Attracted by the brilliance of Staub's results, I obtained a supply of the solution prepared according to his formula, and proceeded to test its effects for myself. In order to avoid burdening my paper with a mass of details, I have tabulated twenty-three cases in which it was employed. The greater number of the patients concerned, both in this and the subsequent series of experiments, were under the care of Mr. W. A. Patchett in the Magdalen wards of the Manchester Union Hospital, and a few were in-patients under Dr. Glascott at the Manchester Royal Eye Hospital. I desire to take this opportunity of gratefully acknowledging my indebtedness to both those gentlemen for the readiness with which they granted me every facility for my investigation, and lent me their personal assistance.

The usual dose employed was that recommended by Staub—namely, five milligrammes (one-fourteenth of a grain) of sublimate in a gramme (fifteen drops) of fluid. The injections were generally given every morning, either in the upper arm or in the gluteal region.

Fifteen of the cases were suffering from constitutional syphilis, and all these had some form of syphilitic eruption. In addition to the cutaneous syphilide, two of them had iritis, and one had gummous tumours, as large as an almond, situated in the substance of the back part of the tongue, which had been in a stationary condition for some months. In every instance the rash disappeared very quickly, the average time being eleven days. The cases of iritis were cured, one after seven and the other after eighteen injections, without any symptom of mercurialisation. The tumours of the tongue had entirely disappeared in ten days.

The cases were not under observation for a sufficient length of time to enable me to say anything about the liability to relapse. In two cases, however, fresh manifestations occurred while actually under treatment.

The average quantity of sublimate required to produce slight tenderness of the gums was about half a grain; and in one instance slight salivation was produced after injecting two-sevenths of a grain. The pain caused directly by the operation was in every instance inconsiderable. The punctures were *always* followed by a certain amount of induration, which had not become absorbed when the patients left the hospital. In one case that I saw three weeks after the last injection there was still a hard nodule at the seat of each puncture. Once or twice there was a little ecchymosis, but in no case either inflammation, abscess, or slough.

[There is perhaps no better solution for subcutaneous injection than three grains of corrosive sublimate to an ounce of water. Dr. Staub, who has paid a good deal of attention to this subject, uses an albuminous solution.]

I conclude, from a series of one hundred injections, that the addition of the white of egg, a distinguishing feature in Staub's solution, is practically useless; while, on the other hand, the presence of small quantities of the alkaline chloride has the beneficial effect of slightly diminishing the pain of the operation.

I now undertook another series of experiments, upon an equal number of patients, with two solutions marked respectively "D" and "S." The former of these was always injected on the right side, the latter always on the left. Solution "D" contained the double iodide of mercury and sodium, prepared according to the directions of M. Bouilhon. Solution "S" consisted of three grains of the bichloride of mercury dissolved in water, to which a little glycerine had been added. I am not aware that this salt has been previously used for subcutaneous injection. It occurred to me that it would be less irritating to the tissues than other preparations of mercury. The results in this series were even more satisfactory than in the former. With both solutions the subsequent thickening was either entirely absent or exceedingly trivial, and in this respect there seemed to be scarcely any difference between the two. In the matter of pain, however, the patients were nearly unanimous in giving the preference to the bichloride, which caused them little or none. With this solution, therefore, the local effects are reduced to absolute insignificance, and I have no hesitation in recommending it for further trial. It is very easily prepared, and is not liable to deterioration, which are additional advantages.

I have been chiefly anxious in this communication to decide two points; firstly, the practicability of the subcutaneous injection of mercury, which I think my researches have established beyond all doubt; and, secondly, the best form of solution,

which, after numerous comparative experiments, I have already indicated.

The larger question of the ultimate value of this mode of treatment cannot be really settled except by observations extending over a series of years. All I shall attempt at present will be simply to enumerate what appear to me to be its special and unquestionable advantages. They are these:—

1. The certainty and rapidity with which the symptoms disappear.
2. The small quantity of mercury necessary.
3. Exactness in the measurement of the dose.
4. Impossibility of disappointment through patients neglecting to take their medicine; &c.
5. Absence of gastric and intestinal irritation.
6. Economy in hospital use.
7. The avoidance of the publicity involved in using baths.
8. The means it affords of rapidly affecting the system in certain grave complications.

I here submit to the profession what I believe to be the first systematic account of the subcutaneous injection of mercury attempted by an English surgeon, and I hope that some of my brethren of greater ability and with a larger field of observation than I can lay claim to will be induced to put this mode of treatment to the proof, and to give us broader data for pronouncing a final judgment on its value.—*Lancet*, May 9, 16, 23, pp. 653, 686, 725.

90.—ON TERTIARY SYPHILIS.

By S. MESSENGER BRADLEY, Esq., Senior Assistant-Surgeon
to the Manchester Royal Infirmary.

Tertiary Syphilis is directly Transmissible.—The evidence which I have to bring forward in favour of this proposition is at present insufficient for demonstration, but having some facts which favour this view, I nevertheless state it, because I think its adoption or rejection has an important bearing upon the question of treatment, and upon the right to marry. If it be proved that in tertiary syphilis the poison is still active, and that tertiary syphilis is not a mere sequel of a preceding disease, then it is probable that the remedy or remedies found serviceable in the earlier periods will prove valuable in this its latest stage. The evidence which I have to adduce is the following. Last year I inoculated a rabbit with small portions of a gummatous tumour which was situated in the calf mus-

cles of a man who was at the time under my care. I killed the rabbit three months later, and found that indurations existed at the site of the old inoculations, and that there were similar deposits in the lungs. Of course it may be said that it was tubercle and not syphilis which was produced in this case, and this may be so; but although I am now undertaking a series of experiments to determine this point, this case is the only one at present which I am able to cite as bearing upon the question. There is, however, evidence of another kind which adds weight to my proposition. Thus, it is generally admitted that parents suffering from tertiary syphilis beget strumous offspring—which is but saying that the syphilitic poison is not sufficiently strong to reproduce itself, but has vitiated the blood sufficiently to determine the production of a strumous stock; and thus struma (rightly, as it seems to me) comes to be looked upon as a sort of bastard syphilis, or, better, as the quaternary form of the disease. Occasionally, however, the children exhibit more direct traces of having inherited the specific poison. I have at present under my care at the infirmary a woman suffering from tertiary syphilis—necrosis of tibiæ, &c.,—who since she has been thus suffering has given birth to a boy. This child is also under my care with interstitial keratitis and osteal and periosteal enlargement of the shafts of both tibiæ, which threaten to become necrotic. His condition has improved under the administration of hydrargyrum cum cretâ, though it must be stated that along with this he has taken cod-liver oil and Parrish's chemical food. It seems probable that the reason why tertiary syphilis is generally regarded as not being directly transmissible is due to the fact that the various tertiary lesions rarely furnish any available secretion which can be received through the usual channels. The very situation of the lesions would render it difficult to communicate the disease from one person to another, even if the attempt were made. It is very likely this which constitutes the difference between the inoculability of the secondary and tertiary form of the disease. It will be an interesting point for future observation to determine, in cases where tertiary syphilis is transmitted, say to the rabbit, whether the transmitted disease assume the tertiary type *ab origine*, or whether it ever in the virgin soil travels through the previous stages of the malady.

Mercury is the True Remedy for Tertiary Syphilis.—The formula commonly adopted is that mercury is the remedy for secondary syphilis, particularly for the earlier manifestations, mercury and the iodides for late secondaries, and the iodides alone for tertiary syphilis, mercury being positively baneful. I venture distinctly to dispute the accuracy of this formula, on the ground of practical experience, and to affirm that, except perhaps in

cases of extreme bone-mischief, mercury may be so administered as not in any way to impoverish the blood or injure the constitution, but with the sole effect of acting as an antidote to the syphilitic virus. I am pleased to find that I am supported in this opinion by the large experience of Professor Lewin, who, in his work on the treatment of syphilis, gives several cases of tertiary syphilis which were cured by the administration of mercury, and by this alone. I do not for one moment deny or decry the value of the iodides, of sarsaparilla, of cod-liver oil, in our treatment of tertiary syphilis—indeed, I am in the habit of employing all these remedies; and every one has had experience of the almost magical influence of iodide of potassium upon some bone affections,—but my conviction is that by these means relief only from present symptoms is afforded; that they perhaps act by improving the general health, and so mask and help to keep latent the syphilis; that, in a word, they may scotch, but do not kill the snake.

I am constantly in the habit of treating tertiary syphilis—affections of the bones, gummata of the tongue or beneath the skin—with mercury, taking care of course never to salivate, with the result of causing a subsidence of symptoms as quickly as with the iodides, and with a much brighter hope of the symptoms being finally banished, never to reappear, as they are almost sure to do after simply giving iodides. It is of course essential to keep up the health to the best possible pitch during the treatment, and for this purpose sarsaparilla (a pint a day if the patient can afford it) and chlorate of potash are both invaluable; but still mercury is the remedy to which we must trust to *cure* the disease.

The mode of administering the mercury is important. The internal exhibition of the drug is frequently ill borne; but either the mercurial vapour-bath or wearing a mercurial belt answers very well. Of the two, the belt is the most uniformly successful in these cases, as exhausting sweating sometimes follows the use of the bath. Neither inunction nor subcutaneous injection are admissible; indeed, I regard the latter plan from pretty extensive experience as eminently objectionable, being liable, in spite of the favourable testimony of Lewin and others, to produce alarming constitutional symptoms and troublesome local sloughing. I have also found that cases of secondary syphilis, treated with the subcutaneous injection, are (probably from the small quantity of mercury employed and the very rapid *apparent* cure) more prone to relapses than cases treated with mercury on the other plans. This, however, is in itself an interesting and important subject, to which I may, perhaps, have an opportunity of returning on a future occasion.—*Med. Times and Gazette*, June 6, 1874, p. 613.

91.—THE TREATMENT OF VENEREAL WARTS AND CONDYLOMATA.

By WILLIAM BERRY, Esq., Workhouse Hospital, Manchester.

The obstinacy to treatment of warts or vegetations of venereal origin situate on or near to the organs of generation is sometimes very great, for in some cases they will, in spite of active treatment, reappear and spread again and again.

The most persistent of this class of growths are those which are small, have a well-defined and broad base, and are covered with a thin cuticle, and thus resemble very closely enlarged papillæ met with in other parts of the body. These warts will sometimes resist the most active agents, such as strong nitric acid, and even removal with scissors will fail to eradicate them, as they reappear in greater numbers with surprising rapidity.

In two cases under my care lately, the daily application of a strong solution of sulphate of copper, the application of fuming nitric acid every second day, and removal with the scissors failed to effect a cure. The saturated solution of sulphate of copper appeared to stimulate them, and, as the nurse remarked, caused them to grow more rapidly.

In these two cases it was often remarkable to see the re-appearance of these growths after the application of strong nitric acid, for no sooner was the yellowish slough removed than they appeared as large as ever; on one occasion the acid was applied after their removal with scissors.

Almost despairing in being able to rid the patients of these pests, I resolved to try the acid nitrate of mercury (liquor acidus hydrargyri nitratis), though with little faith in its efficacy, after having failed with nitric acid.

In both cases (females) the warts grew on the perinæum, around the arms, and on the skin and mucous membrane of the labia majora. I oiled the parts around and applied the acid nitrate of mercury freely, by means of a firm pledget of lint, intending to do so again daily if required. Next morning, however, to my surprise, the warts had become much shrunk and appeared to be covered with a yellowish white slough, the patients complained of feeling very sore, and had been pained since the application. Poultices of linseed meal were now applied, and when the parts were cleaned the warts had almost completely disappeared (a second application removing them), and the skin where they had been was quite healthy.

In some cases, where the warts are one large granulating mass, giving forth an offensive discharge, removal with the *écraseur* will be required; but in those cases where they cover a large surface the application of the acid nitrate of mercury will be found to be the best remedy.

What part the mercury plays in its caustic or escharotic properties I am unable to say, but certain it is that the remedy is superior to strong nitric acid.—*Med. Press and Circular*, June 10, 1874, p. 487.

92.—CASES ILLUSTRATING THE DIURETIC ACTION OF COPAIBA RESIN.

(Under the care of Dr. MOXON, at Guy's Hospital.)

For the following interesting notes we are indebted to Dr. Ernest Field:—

E. S. F., aged $11\frac{1}{2}$ years, was admitted into the clinical ward on February 18, 1874, suffering from mitral regurgitation. On admission she was extremely ill; there was a large amount of dropsy over the whole of the body, much blueness of the face, and a distinct systolic apex-bruit. Owing to the amount of congestion present, as shown by the condition of the lips and face, a few ounces of blood were removed by venesection; this did not appear, however, to materially relieve the patient. Diffusible stimulants and tincture of digitalis were given at the same time freely, the dose of the latter being gradually increased to twenty minims every four hours. Although at first it was not thought by anyone who saw the girl that she would live twenty-four hours, her condition gradually improved; until, on February 23, there was no bruit to be heard at the apex, the congested appearance of the face had vanished, and the child was cheerful and generally comfortable. The dropsy, however, had not diminished; and at this time only sixteen ounces of urine were being passed in the twenty-four hours. Under these circumstances ten grains of the resin of copaiba were added to the digitalis mixture; in two days the amount in twenty-four hours was thirty ounces, but owing to the sickness produced by the medicine the resin was discontinued. On February 26 the amount of urine passed in the twenty-four hours was two pints, and copaiba resin was detected in it.

The quantity passed daily remained at this point until March 7, and there was no great diminution in the amount of dropsy, the legs resembling "pillows" (to use the words of the physician in charge). The general state of the patient seemed satisfactory, if only she could get out of her water-logged condition. Accordingly, it was determined to make another trial of the copaiba resin. The digitalis mixture was discontinued, and one containing copaiba resin substituted.

On March 9 the quantity of urine passed was four pints; on the 10th, four pints seven ounces; on the 11th, four pints. By the 15th the whole of the œdema had disappeared, and the

appearance of the limbs in their shrunken, wasted condition was very striking. The drug was now discontinued, and cod-liver oil with some iron-wine was ordered, as the little patient was in an extremely cachectic, anæmic state.

Up to March 20 she still continued to pass about four pints of urine daily—an effect which then was not desirable, as she was not in a condition to part with fluid to such an extent in her enfeebled state. It is worth noting that at this time no copaiba resin could be detected in the urine. By March 23 the amount of urine passed had returned to its normal quantity. The patient is now (April 10) about to be discharged.—*Medical Times and Gazette*, May 9, 1874, p. 501.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

93.—ON THE INDUCTION OF PREMATURE LABOUR.

By Dr. JOSEPH GRIFFITHS SWAYNE, Physician-Accoucheur to the Bristol General Hospital.

[We pass over the first part of Dr. Swayne's paper, which consists of notes of twenty cases in which premature labour was induced, and proceed to his description of his mode of performing the operation.]

In my two first cases before the year 1850, labour was induced by the old-fashioned methods, viz., by puncturing the membranes and giving ergot in No. 1, and by detaching the membranes around the os in No. 2. After the time mentioned, having read Sir J. Simpson's memoirs on the subject, I always commenced the operation by dilating the os uteri mechanically before puncturing the membrane—a method which is much superior, because it more exactly imitates the natural process of labour. To puncture the membranes before there is any dilatation of the os is to put the cart before the horse, and to invert the process of labour; and, therefore, it is no wonder that such a proceeding, especially when combined with the use of ergot, is more apt to endanger the life of the child from compression of the cord.

Accordingly, in all my other cases (with the exception of No. 4), labour was invariably commenced by dilating the os uteri. In No. 4, the condition of the mother was so critical, that very prompt delivery was necessary, without reference to the child, and there was already some dilatation of the os uteri.

The instruments ordinarily employed for the purpose of dilating the os uteri are tents of sea-tangle or compressed sponge, and the elastic India-rubber bags recommended by Dr. Barnes. My own experience leads me to prefer the sponge-tents, and I now almost invariably employ them, and insert them in the following manner. The patient being in the ordinary obstetric position, the fore and middle fingers of the left hand, well oiled, are to be passed up to the os uteri externum, which is generally sufficiently open to admit the point of one,

and sometimes even of both, fingers. A small-sized sponge-tent, about the size, for instance, of an ordinary radish, is to be mounted on a proper stilette, made for the purpose, and passed along the groove between the fore and middle fingers of the left hand, until its point is inserted into the os uteri. The tent is then to be pushed on until a very little of it, not more, perhaps, than a quarter of an inch, projects into the vagina beyond the os uteri. If the os be so high that it cannot well be reached by the finger, the tent may be inserted through a full-sized tube speculum. If also it be very little open, a tangle-tent may be found more convenient to begin with. Care should be taken to push the tent up sufficiently far, as it should be remembered that, two months or so before delivery, there is often a considerable interval between the os uteri externum and internum; otherwise the tent may not pass through the os internum. After the tent is introduced, a piece of soft silk handkerchief may be passed up as a plug to keep it *in situ*. On the day following, this tent should be removed and a large one inserted, and on the next day a tent of the largest size. Generally two, or at most three, tents are required to complete the full dilatation. At this time, probably labour-pains will set in regularly, and the membranes will rupture. Should this not be the case, the membranes may be punctured, and the most convenient instrument for this purpose I have found to be a gum elastic male catheter, of which the end has been cut off, so that the stilette can be protruded at pleasure. Should the pains still be weak or ineffectual, ergot may be given.

Before it was the custom to impregnate sponge-tents with carbolic acid, they used to become very offensive if left in the vagina more than ten or twelve hours, and there was some risk of septicæmia in consequence. I, therefore, never was accustomed to leave a tent *in situ* more than twelve hours. In this time, however, the tent will not dilate to its full extent. I now always leave each tent twenty-four hours *in situ*, and I do not find that there is any risk of septicæmia when carbolised tents are used.

On the whole, I prefer carbolised sponge-tents very much to the elastic bags, because they effect the process of dilatation more steadily, continuously, and gradually, and therefore imitate more exactly the natural process of dilatation, which frequently occupies several days from its first commencement. As a general rule, it is not desirable to dilate the os too rapidly, especially when the time for that operation falls considerably short of the full term, for then there is a proportionally greater thickness of the os uteri to overcome.

Unless there be some special circumstances present which

render prompt delivery necessary, it is never advisable to induce premature labour with that rapidity which Dr. Barnes enumerates as one of the advantages of the elastic bags, when he remarks, "that it is just as feasible to make an appointment at any distance from home to carry out at one sitting the induction of labour as it is to cut for the stone." This is, no doubt, one great advantage which we can derive from the use of the elastic bags; but still it is one of which we should do well not to avail ourselves, except under very exceptional circumstances. The chief objection, I think, to the elastic bags, is the difficulty of introducing them. Unless the os uteri is tolerably low, and open enough just to admit the tips of two fingers, it will be much safer to commence the dilatation with a tent of tangle or sponge.

The opposite extreme, of occupying too much time in the process of dilatation, is also to be avoided. If the process occupy more than three or four days, the continual pressure of a foreign body in the os uteri may cause considerable irritation, or even inflammation of the part, which may produce ulceration and softening, and thus give rise to very untoward results, especially if the os be subjected to any undue strain afterwards during the labour. In one of my cases (No. 5), I believe that this caused led to a rupture of the neck of the uterus, and consequently to a fatal result. In this case the child lay transversely; the os was very high, very little open, and very difficult to reach; and I think it very probable that most of the dilators were not passed far enough through the os uteri internum to act very effectually. In this way ten days were consumed in the dilatation, and when the labour came on the child was expelled very rapidly in a doubled condition, and thus a rupture of the neck of the uterus was occasioned, which, although it did not pass through the peritoneum, was sufficient to cause death. There was, no doubt, an unfortunate combination of circumstances in this case; yet it will always be well, I think, if the os be not well dilated in three or four days, to desist from further efforts by these means, and to adopt some other method of inducing labour; or even to give up all attempts for the present, and recommence at the end of a week or ten days. I have sometimes known the labour come on quite unexpectedly, and apparently spontaneously, many days after all efforts to induce it had been abandoned. This happened in one of my own (No. 11) cases.

There is another very effectual way of bringing on premature labour, of which I have had no experience—I mean the intrauterine douche. I have never tried it, because I have been, on the whole, well satisfied with the sponge-tents, and I

have been deterred also by the alarming and even fatal results which have occasionally followed its use even in the most skilful hands.

The time occupied by a premature labour artificially induced is very variable. In my own cases, the shortest time was six hours (No. 8), and the longest sixteen days (No. 11). In the first, the woman was in such a critical condition, owing to constant vomiting, that it was necessary to deliver as quickly as possible; and here I experienced the very great utility of Dr. Barnes's elastic bags in causing rapid dilatation. In the second, after trying in vain for several days to induce labour by tangle and sponge-tents, I desisted from further efforts. About ten days afterwards, labour came almost as if spontaneously, and terminated very favourably for both mother and child. On the whole, the average time occupied was about three days.—*Brit. Medical Journal*, Aug. 8, 1874, p. 167.

94.—HYDROCHLORAL BY THE RECTUM IN THE VOMITING OF PREGNANCY.

Dr. SIMMONS, the chief surgeon to Ken Hospital, Yokohama, Japan, remarks (New York Medical Record, June) that he does not remember having seen hydro chloral, by the rectum, recommended in the vomiting of pregnancy. Should this application of it, however, not be new, the results of the following observations may still have their value for or against the conclusions already arrived at. Aware of the suddenness with which this symptom sometimes ceases, after all hopes of saving the mother, without emptying the uterus, have failed, and that, too, without being able to attribute it to any of the numerous remedies which usually have been tried, *except the last*, he has waited for a third case before venturing a decided opinion as to its value. Although he saw these cases only in consultation, especial care was taken to obtain their correct histories. The following is one of his cases:—Patient aged 30; third child. Commenced excessive vomiting during fifth week of pregnancy, which continued, with the usual intermissions, till the tenth week, when we saw her. For several days previous the nausea and vomiting had been almost constant, both day and night. She had become very much emaciated and unable to sit up, even in bed, not having retained any nourishment on the stomach for several days. All the usual remedies had been tried, such as oxalate of cerium, hydrocyanic acid, hypodermic injections of morphine, &c., but with little benefit. He suggested the administration by the rectum, morning and evening, of thirty grains of hydrochloral on mucilage, and this to be increased if there was

no improvement, or if the specific effect of the medicine was not too decided. An amelioration of the symptoms was obtained by the first injection, and a still more satisfactory one followed the administration of the second. The second day's use of the remedy arrested the vomiting, except at long intervals, and on the third day both nausea and vomiting ceased entirely. There was no return of the symptom. Some nourishment was taken and retained, even on the second day. From this time the patient rapidly gained strength, and was delivered of a healthy child. Should other opportunities offer for a trial of this plan of treatment, he has decided to commence with larger doses, being convinced that a decided impression, produced by the medicine at first, will require its repetition but two or three times to put an end to the disease, for the time at least. He believes that hydrochloral, administered in this manner, will relieve most cases of nervous or sympathetic vomiting, where there is no inflammation especially. Even in strangulated hernia, on theoretical grounds, it ought to act well, not only in checking the vomiting, but in producing relaxation. He would give it a trial also in cholera.—*Practitioner*, Sept. 1874, p. 206.

95.—A CASE OF TWINS WITH DOUBLE PLACENTA PRÆVIA (PARTIAL), AND BOTH CHILDREN PRESENTING TRANSVERSELY.

By Dr. ANGUS MACDONALD, Lecturer on Midwifery at Edinburgh.

In a paper read in October last year before the Obstetrical Society of London, and published in the last Heft of the *Archiv für Gynäkologie*, Dr. Duncan has advanced an entirely new view regarding the natural mechanism of the separation of the placenta from the uterine surface in cases of placenta prævia. In that contribution Dr. Duncan maintains, and supports by arguments, which to my mind at least seem incontestable, that the spontaneous detachment of the placenta when it presents is not due to contraction or diminution in all directions of the placental area, but to stretching of its uterine attachment through the opening of the lower segment of the uterine ovoid, to form a passage for the child.

In following out this view, Dr. Duncan has defined, more rigidly than Dr. Barnes and others who have travelled this ground before him, the exact limits of the spontaneous detaching area. According to him, the placenta, in an ordinary case of complete placenta prævia, with a full-grown child at full term, ceases to suffer any further separation when the dilata-

tion of the lower segment of the uterus attains a diameter of 11 centimetres, *i.e.*, nearly $4\frac{1}{2}$ inches. This amount of dilatation, as the result of careful measurements made by Dr. Duncan, is shown to coincide with the circumference of a circle whose plane is at right angles to the longitudinal diameter of the uterus, and whose diameter measures 11 centimetres, whilst every part of its circumference, measured along a meridian line of the uterus, is 6 centimetres distant from the central point of the internal os uteri.

It follows, therefore, that in a case of placenta prævia such as I have been describing, if the head had presented, hemorrhage, due to separation of the placenta, would certainly have ceased some time *before* a segment of the spheroid, formed by the lower extremity of the uterus, with a chord of 11 centimetres, and an arc of 12 centimetres, had been obliterated by the natural dilatation.

I say *before* advisedly, because, even though the case had gone on to full time, such a segment would be the maximum amount of dilatation necessary; whereas in the labour which we have been considering, the smallness of the children rendered the amount of dilatation necessary considerably under a segment with a chord of 11 centimetres.

We are thus, through the labours of Dr. Duncan, brought to such a degree of certainty, or at the very least of approximate certainty, in respect to the natural limit of bleeding in cases of placenta prævia, which is a very great advance upon the bewilderment induced by theoretical ideas regarding indefinite and indefinable zones.

But notwithstanding all that has been previously written upon this subject, and making full allowance for the above-mentioned and the three other able and more recent contributions of our President to the Edinburgh Medical Journal and the British Medical Journal, in regard to the origin and mode of arrestment of this kind of hemorrhage, much has still to be done, as the whole subject is still enveloped in very much mist.

If I might venture a criticism upon Dr. Duncan's theory of the origin and mode of arrestment of bleeding in such cases, which has been lately put forward and ably advocated by him, it is that I think he has paid too little attention to the curling arteries as a source of such bleeding, and too much to the venous sinuses in the placental area.

Dr. T. Snow Beck, in the Obstetrical Journal for December, 1873, and January, 1874, has endeavoured to revive the views originally brought forward by Dr. F. W. Mackenzie on this subject. He maintains, supporting his arguments especially by reference to Dr. Mackenzie's experiments, that in bleeding

from the uterine surface the bleeding is arterial and direct. On the other hand, Dr. Duncan would have it an indirect oozing, the arteries inside of the uterine wall bleeding first into the sinuses, and then the sinuses discharging their blood into the cavity of the uterus through its free surface. Dr. Duncan does not hold that the bleeding is regurgitant, in the sense that it is a reflux from the veins leading from the uterus towards the heart back again into the uterine sinuses.

Now, though I have not sufficiently studied this exceedingly intricate subject so as to be able to take up the strongly-affirmative position defended by Dr. T. Snow Beck, I must confess that my leanings are towards his views; and I consider the facts, experiments, and arguments advanced by him, deserve a larger amount of attention from the thoughtful accoucheur than they have as yet received. It would be a great gain if the profession thereby were only led to abandon the exceedingly unphysiological and provedly hazardous treatment of post-partum hemorrhage by means of injections of perchloride of iron, and to get free of belief in the dangerous nature of the exceedingly effective—and, as I am glad from extended experience for several years back to be able to testify, in perfect accordance with the views of Dr. T. Snow Beck—completely innocuous mode of arrestment of hemorrhage post-partum by injection of cold water into the uterus.

But this is rather departing from my subject. In reference to the course of the bleeding, I would beg to state that the late Professor Goodsir—than whom, I am sure the members of this Society will allow, a more patient, more able, or more careful observer never lived—used in his lectures on the anatomy of the uterus to substantiate the views of Owen and of Simpson in regard to the platform arrangement of the uterine veins, and to describe the manner in which the successive platforms opened into each other obliquely, with the free edges projecting in such a manner as to act substantially as valves, and thus to prevent the escape of blood from a higher platform to a lower. I feel convinced that though it is difficult to understand how such small vessels, numerous though they be, as the uterine curling placental arteries, may give rise to such severe bleeding, both in connexion with placenta prævia and in hemorrhage post-partum, yet the difficulties, on the other hand, to account for the arrest of hemorrhage from the open uterine sinuses without the contraction of the organ, are still more difficult to meet.

In regard to this point, I do think that the existence of some such anatomical arrangement of the veins, such as Owen and Goodsir pointed out, gets us more rationally and easily over the difficulty than to trace it to the obstruction due to a change

of shape in the uterine sinuses effected in the course of the dilatation of the uterine placental area, consisting in the elongation of them in the lateral direction, and the approximation of their walls in the longitudinal direction, as Dr. Duncan urges. I do not think that Dr. Duncan, even when backed up by the great and justly-respected name of Professor Tait, proves that point well. This part of Dr. Duncan's paper is to me not at all convincing.

Before such change in the shape of the discharging orifice could be effective as a hemostatic means, I feel certain that we would need a diminution of the discharging area in all directions; for I cannot imagine the mere friction of the opposed surfaces of openings, so large as those sinus openings are, to be able to resist the degree of tension, often great, to which the maternal blood in the uterine walls is subjected, both in placenta prævia and otherwise. In the case of the small curling arteries again, supposing them to be sources of hemorrhage, we have the greater contractility of their walls, the fact, also, that the separation is by tear and not by cutting, and the exceedingly small diameter of the vessels, uniting, to afford conditions specially favourable for the process of nature's hemostatics. Only in cases where the general blood-pressure was specially exalted, or in which the retentive power of the abdomen (so ably and convincingly urged by Dr. Duncan as one of the means by which the bleeding is arrested from the free surface of the uterus, in cases of placenta prævia) was defective, would the natural hemostatic means afforded by the torn surface of those arteries be expected to fail, provided we could account for occlusion of the sinuses by any certain means. That the veins inside the uterus have such an arrangement as could make their various platforms incapable of transmitting blood from a higher to a lower level is my contention, supported by the experiments of F. W. Mackenzie, and the dissections of Owen and Goodsir.—*Edinburgh Medical Journal*, May 1874, p. 995.

96.—ON SOME IMPROVEMENTS IN THE SINGLE AND
DOUBLE CURVED FORCEPS AND THEIR USE
IN MIDWIFERY PRACTICE.

By Dr. THOMAS MORE MADDEN, lately Examiner in Midwifery
in the Queen's University in Ireland.

[There can be little doubt that the dangers of childbirth have been lessened of late years by the more frequent and judicious use of the forceps, in the construction of which instrument it is the object of this paper to describe some modifications.]

Upwards of a century ago the forceps appears to have been very generally and indiscriminately resorted to in midwifery practice in this country. A reaction in professional opinion then set in, extending against even its legitimate use, and so far did the prejudice go that for many years the only instrumental assistance afforded in cases of difficult labour was craniotomy, or, as Sir James Simpson well said of this operation and its results, "Cold steel for the child, and mercury for the mother."

The comparative advantages of the straight and double curved forceps have been warmly disputed from the days of Smellie and Leveret to the present time. The controversy, although recently revived, is interesting only on account of the ability with which it has been conducted; as there can be no just or useful comparison between objects which, like the instruments now referred to, are essentially different, and adapted for distinct purposes: the short forceps being intended for cases where the head is detained, generally by inertia, in the cavity of the pelvis, and where only slight traction is required to effect delivery; the long forceps, on the contrary, is not only a tractor, but is also a powerful lever by which an impacted head may be moved, as well as a compressor, by which its size may be sufficiently diminished to admit of its passage through the pelvis.

I shall now describe the single and double curved forceps which I exhibited at the last meeting of the British Medical Association in London. The first, or straight forceps, is designed for ordinary cases of delay in the second stage of labour, being a very powerful tractor, although remarkably light and small. Its weight is only eight ounces, and its length ten inches, the blades being six inches, and the lock and handles four inches. The greatest space between the blades when locked is two inches and seven-eighths, and between the points one inch and a quarter. Particular attention has been paid to the avoidance of any sharp edges or points. The curve of the blades is by no means as sharp as is the case in the ordinary forceps, and commences half an inch above the lock, the intervening shank being beveled internally, and bent outwards, so as to form a space for the finger of the operator. The fenestra, which extend throughout the blades, are very wide, so that the scalp may protrude and cover the rims, thus protecting the maternal passages during the extraction.

Having now employed this instrument in upwards of a hundred cases, and also received accounts of its use in other countries from several former pupils of the hospital, I venture to recommend it as being much easier of application and better

adapted for ordinary cases of delay in the second stage of labour than any of the longer or double curved forceps.

It has been proved that the dangers of the puerperal state bear a certain relation to the length of the second stage of labour, the physiological duration of which cannot be safely much prolonged or much shortened. It is obvious that the natural process of childbirth should never be unnecessarily and rudely interfered with, by rash and premature recourse to instruments. But, on the other hand, it cannot be allowed to run on indefinitely until the parturient woman is worn out by protracted suffering, without greatly increased risk of puerperal fever and other diseases of childbed, as well as of more immediate danger from post-partum hemorrhage, the result of exhausted uterine contractility. Inflammation and sloughing of the vaginal walls, leading to vesico-vaginal or recto-vaginal fistulæ, rupture of the uterus and the death of the child from the long-continued pressure to which it is subjected in protracted labours, are also amongst the occasional consequences of undue delay in affording timely instrumental assistance when it is required. In the great majority of cases this purpose may be easily accomplished by my short forceps, which is not only efficient but portable, and may be applied without causing the patient any pain, or, if necessary, even without changing her position in the bed.

My modification of the long forceps, although a somewhat complicated-looking piece of mechanism, is intended to alleviate the use of instruments far more formidable, being especially designed to effect safe delivery in cases in which a few years ago the only operation resorted to was one involving the death of the child, and occasioning greater danger to the mother than now attends the cautious application of the long forceps, by which both may be saved.

This instrument combines the action of a powerful lever and tractor with those of a compressor, the power of which may be exactly regulated in each case at the will of the operator, and which may thus vary from the most gentle and almost imperceptible pressure on the child, which is generally sufficient, to an amount of compressing force which is very seldom necessary, and of which we should be slow indeed in availing ourselves. The weight of this forceps is twenty-six ounces, and its length eighteen inches. The blades occupy ten inches, of which seven are taken by the fenestrated portion and three by the shanks; the greatest space between the blades when locked is about two inches and three-quarters; the lock and handles are eight inches long. The latter are movable, and thus it is made portable, and may be applied in the ordinary obstetric position. When a greater degree of power is required, the handles are adjusted,

and the patient is then placed on her back for the operation. To the handle a rack and pinion is attached, and these are connected by a screw which regulates the amount of pressure exercised on the child's head. The traction power of the instrument is increased by strong shoulders, and, owing to the length of the shanks, there is no danger of nipping the soft parts during the locking. The curvature of the blades is less acute than in ordinary forceps, in which it is generally that of a section of a circle so much larger than the foetal head that the instrument is in contact with the head at only two points, on which excessive pressure must be made, and from which it is liable to slip; whereas in my forceps the blades were carefully moulded on a number of average-size foetal heads, until a curve was obtained which allows the most perfect apposition between the blades when *in situ* and the child's head. Thus the possible injurious effect of the compressing power of the instrument, as well as the danger of its slipping, are both reduced to the minimum.

The power of the long forceps as a compressing instrument is not generally sufficiently appreciated. I have elsewhere discussed this question, and have shown that it is possible to compress a child's head with the forceps sufficiently to enable it to be extracted through a pelvis rather smaller than its natural diameter. No argument is necessary to prove the extraordinary compressibility of the foetal head. Every tyro in midwifery is cognisant of the extraordinary manner in which the child's head may be moulded or lengthened out by the pressure to which it is subjected during protracted labour, without any permanent injury, and the intention of using a compressing forceps such as mine is to imitate and supplement this natural moulding process in cases where the *vis a tergo* does not suffice to overcome some obstacle to delivery.

Living children have been extracted through pelves measuring three inches in their widest diameter; therefore it does not follow that because a woman's pelvis is small, or even somewhat deformed, she must be delivered by craniotomy or cephalotripsy. On the contrary, I have myself recorded cases which prove that even under these circumstances the forceps may sometimes be used with success; and in no less than twenty-nine of my forceps operations there was some degree of disproportion.

Amongst the other cases in which the instrument may be resorted to are difficult labours, where the foetal head is prevented by malposition from passing the brim. I have also employed it in deliveries complicated by convulsions, accidental hemorrhage, prolapse of the cord, premonitory symptoms of rupture of the uterus, and in one fatal instance of this accident.

When the symptoms are so urgent as in these cases, there is

no doubt as to the propriety of delivering with the forceps, as the os uteri can be sufficiently expanded; but nothing short of such necessity justifies instrumental assistance before it is fully dilated. Nor should this instrument be ever resorted to in cases of simple inertia until other means, such as change of position, friction over the fundus, ergot, and stimulating enemata have failed.

With regard to the manner of using these forceps I may add a few words, as they each differ in several respects from the instruments generally employed. In the first place, the application of the forceps should, when possible, be previously sanctioned by a consultation. Immediately before operating, a dose of ergot should be given, and the catheter passed. The patient may then be put under the influence of chloroform, although this is seldom necessary when the short forceps is used. In order to apply my straight forceps the patient should be placed on her left side, with the hips projecting over the edge of the bed. The operator now introduces the index and second finger of his right hand into the vagina until he reaches the child's ear, between which and the symphysis pubis the upper or right-hand blade of the forceps, lightly held in his left hand, is to be gently insinuated until the lock presses on the perineum, where it is to be supported by an assistant. In a similar manner the left-hand blade is to be passed in, held of course in the opposite hand, and passed around the child's head till the handles come in contact, and the locking is effected without any force whatever. The extraction of the head may be now accomplished by drawing very slowly and gently in the direction of the curve of the pelvis, first downward and backward, then downward and forward, and lastly, as the head passes through the public arch, directly forward, until the head protrudes from the vulva; when the instrument should be unlocked and withdrawn, and the delivery completed by pushing out the head by pressure with the hand from the coccyx over the perineum.

These directions, with a few differences, may be followed also in the use of my long forceps. The same preliminaries being adopted, the patient should be placed on her back in the lithotomy position if possible. If, however, as sometimes happens, it is found difficult to overcome the prejudice here generally existing against this position, the instrument may be used in the same manner as the short forceps, the patient lying across the bed on her left side, with the hips projecting. In using the long forceps it will be found easier to introduce the left-hand blade first, whereas with the short instrument the upper blade is first applied. Before passing it the instrument may be locked so that the operator can see that the blade he

selects first will look forward when *in situ*, or in other words that its curve will correspond with that of the pelvis. When the head is high up above the brim the hand should be introduced till the ear is felt, not that it is always necessary to apply the long forceps in this situation, but it is always necessary to guard the uterus from the instrument, which should be cautiously passed between the operator's hand and the child's head in the transverse or oblique diameter of the brim of the pelvis. The introduction of the second blade and locking require no further observation. When properly adjusted, though locked, the handles of this instrument should not come closely together; they may therefore be very slowly and gently brought nearer each other by the screw at their extremity, but no force is to be used, nor any violent effort made to approximate them under any circumstances. In the extraction with this forceps there is nothing to be added to what has been said on that point in reference to the short forceps, except that the natural direction of the handles will point out the direction in which we are to pull at each stage of the operation, so that we may use our traction force in the direction of the axis of the pelvis. In no stage of either of these operations is any violence ever necessary or justifiable.—*Lancet*, June 20, p. 865.

97.—ON THE USE OF PERCHLORIDE OF IRON IN POST-PARTUM HEMORRHAGE.

By DR. LOMBE ATTHILL, Vice-President of the Obstetrical Society of Dublin.

It is much to be regretted that the discussion which has hitherto taken place on this point has not been carried on with greater calmness. The question to be decided is one to which too great weight can hardly be attached. If the perchloride of iron be a remedy as safe as it is avowedly efficacious, its use should without doubt, be encouraged. If, on the other hand, its employment be fraught with such danger as is maintained by some, we should, with equal distinctness, reprobate its use under almost any circumstances. This being the case, it is a matter of deep regret that some of the writers who have taken part in the discussion—not yet concluded—should allow themselves to be drawn into the grave fault of substituting assertions for facts, uncourteous denials for proofs, and to forget themselves so far as to indulge in pungent sentences, which, however clever as retorts, savour more of personal antipathy than of scientific argument. Such papers, however agreeable to read, fail utterly in deciding in any way the value of the plan of treatment under consideration; that can be effected

alone by carefully recording the cases in which the treatment has been tried, and the results. Such records should state the condition of the patient at the time when the styptic was injected, its immediate effect, and the termination of the case. The points to be decided, in my opinion, are three—

1st. Do cases of post-partum hemorrhage occur which we have reason to believe would terminate fatally, or at least in which life is seriously endangered, notwithstanding the judicious use of the remedies ordinarily employed for the arrest of hemorrhage?

2nd. Is the injection of a solution of the perchloride of iron of itself a dangerous remedy? and

3rd. If so, are the dangers likely to follow its use such as to outweigh its obvious advantages as an agent capable of effecting with almost certainty, the further loss of blood?

In order to aid in some degree in the elucidation of these most important questions, I shall detail the particulars of the cases in which the solution of the perchloride of iron has been injected into the uterus for the arrest of post-partum hemorrhage in my private practice, hoping that my doing so may induce other members of the Society to bring forward the facts connected with similar cases, and that as a result, some practical inferences may be deduced on which sound rules for future practice may be based.

This I will assume as proved, that the perchloride of iron applied to the interior of the womb is an efficient hemostatic. To my judgment this much is conclusively proved.

I think the first of the three questions I have put down for discussion will be answered in the affirmative all but unanimously. Dr. Barnes and Dr. Gream state that they have "never seen a woman die from post-partum hemorrhage when under their own care from first to last." I regret to say my practice has not been equally successful. I have lost at least one patient of post-partum hemorrhage, whose bed-side I never left from the termination of the first stage of labour till death ensued, and whose labour, up to the occurrence of the hemorrhage, which did not set in till after the expulsion of the placenta, was in all respects easy and natural. In her case I exhausted all the ordinary methods employed for the arrest of hemorrhage, but my patient died before my eyes. I admit, however, that I did not rely on the use of brandy to the extent recommended by Dr. Gream. I gave it freely, both by the mouth and by the rectum. No doubt Dr. Gream would say the fatal result followed simply because "I did not give enough." But if I did not give it by the half-pint, I gave it by the ounce, and the large doses of alcohol I did give were nearly useless, because they were rejected as fast as swallowed. Moreover, I

regret to say, I did not inject the perchloride of iron. It was the first case of severe hemorrhage which occurred in my practice after that method had been brought under my notice by Dr. Barnes; and, like many of my brethren now-a-days, I feared to use this, to me, new and powerful remedy. I now firmly believe that to this timidity the death of my patient—a young wife and a young mother—was due. I feel that she might still be alive if only I had used a remedy I knew of, but had not courage to employ. This, however, I have to compensate me, that though since then I have stood beside the bed of more than one whose life seemed to me in greater peril than hers, to whom I have just alluded, no such scene as that I then witnessed followed, nor do I believe it ever will again. The lesson I that day learned taught me the utter inutility of the “ordinary means at our command” for the arrest of post-partum hemorrhage in a certain class of cases.

I shall now lay before the Society the particulars of the following five cases which have occurred in private practice. I should add that I have seen the perchloride of iron used in other cases, but as they were not those of patients directly under my own care, I do not refer at present to them, but shall merely premise that, so far as I am aware, no unfavourable symptom subsequently occurred in any of them.

Case 1.—This was, I believe, the first occasion in which the perchloride of iron was used in Ireland. The patient was the wife of a medical man, who, never robust, suffered much from sickness of the stomach during her pregnancies. She was an example of that numerous class of women whose health is often permanently injured from the habit, frequently I believe, acquired, of taking an altogether insufficient quantity of food. Her husband assured me that during the whole of her married life, the quantity of food consumed by her day by day was so small that, to use his own words, “she ate nothing.” During this her fourth pregnancy her appetite, always small and capricious, was further impaired by constant nausea and frequent vomiting. Some hemorrhage occurred on the morning of the 11th July, 1862. She was then in the eighth month of her pregnancy, but the loss was so slight she did not think it needful to send for me, especially as there were not any pains present. Her nurse-tender, however, calling by chance, she kept her in the house: after a time sharp pains set in, and the labour terminated rapidly in the birth of a dead child. Slight hemorrhage continued during the progress of the labour, which terminated under the sole charge of the nurse-tender. The placenta was expelled almost immediately on the birth of the child. The hemorrhage however, continued, and the husband, becoming alarmed, sent for me. I was from home, and the

messenger proceeded to Dr Kidd's. He has given me the following note of the case:—"The lady lived some distance out of Dublin. Her husband, a medical man, wrote to you saying she had been delivered some hours, and was very weak from the effects of post-partum hemorrhage, which was still going on. As you were from home, the note was sent to me, and I at once visited her, taking with me a syringe and a solution of perchloride of iron, with which I had for some time been provided, in consequence of reading Dr. Barnes's papers on the use of the perchloride. When I arrived I found the lady in a state of extreme prostration; the surface was cold; she was almost pulseless, throwing her arms about, and begging to be allowed to set up and get air. A skilful nurse was holding the uterus. For some time after my arrival I took charge of the uterus myself, and found, as the nurse had said, that the moment the pressure was removed the uterus relaxed and the bleeding returned. It was evident she could not bear the loss of more blood, and that there was no time to be lost, so I proceeded to inject the solution of the perchloride of iron as directed by Barnes; this at once checked the bleeding; the uterus contracted and remained firm. I applied a bandage, gave brandy and opium, got hot jars and flannels to the surface, and watched her closely till reaction was fully established; by this time you had arrived, and took further charge of the case."

The condition of the patient when I saw her, fully confirmed the account given by Dr. Kidd; reaction was established, but the patient was still in a most precarious condition. She was greatly exhausted, the exhaustion being kept up by incessant vomiting, which had set in immediately after the birth of the child, and which, notwithstanding the arrest of the hemorrhage, continued with unabated violence. Brandy, opium, ice—all were tried, and all failed to check this at first distressing but now alarming symptom. Tinct. opii injected into the rectum was equally inefficacious; at last I thought of trying the hypodermic injection of morphia—a remedy I had at that time but little experience of. I injected half a grain of the acetate of morphia; this had an instantaneous effect. She did not vomit once afterwards, and from that moment improved and made a good recovery. I have since then attended this lady in two confinements, both of which were perfectly normal.

As I have already mentioned, this was the first occasion in which the perchloride was used in Ireland for the arrest of post partum hemorrhage, and I firmly believe that to Dr. Kidd's promptitude in using it this patient owes her life.

Case 2.—Mrs. C., a pale delicate-looking lady, aged twenty-three, with an anemic murmur, audible at the base of the

heart, gave birth to her second child on the 16th October, 1869, after a short labour of but four hours' duration. The placenta was expelled in about fifteen minutes after the birth of the child. During this interval I kept my hand constantly on the fundus of the uterus. It contracted firmly, and I applied a binder. Shortly after a small stream of blood was observed to trickle down from the vagina. On loosening the binder, I found that the uterus had become relaxed. The application of my hand caused it to contract immediately, and some clots were expelled. This alternate contraction and subsequent relaxation of the uterus, so often seen in the case of women with relaxed muscular tissue, was repeated several times. The total quantity of blood lost was not large, but it told greatly on this delicate and fragile lady. Cold judiciously applied, ergot, friction, &c., failed to produce more than temporary contraction of the uterus. The fatal case of post-partum hemorrhage I have already alluded to was fresh in my memory; it had occurred in my own practice and in a very similar patient but a few months before. The successful result of the case just detailed had lessened, if it had not altogether removed, my dread of the action of the perchloride of iron. My patient was in a critical state—indeed, to one in her state of health, of great danger. I resolved to use the perchloride of iron, and accordingly injected about an ounce of the liquor of the perchloride of iron, B. P., diluted by the addition of three ounces of water, into the uterus. The effect was instantaneous—the uterus contracted much more firmly than before, and *did not again relax*. Not a drop of blood was subsequently lost. This lady recovered without a bad symptom, and has been confined twice since.

Case 3.—In July, 1870, I was requested to visit a lady, the wife of a medical man, who expected her first confinement in about a month's time. Her general health was very bad; she had suffered much during her pregnancy from sickness of the stomach, which, though it had now ceased, had reduced her strength greatly. She was unable to eat any solid food, her appetite, such as it was, being extremely bad and capricious. She was very large, and quite unable to take exercise. She suffered besides from severe pain in her back, and along the margin of the false ribs. Her complexion was sallow, and her aspect indicated much suffering. I felt anxious as to the result of her confinement. Labour set in during the night of the 1st August, 1870; the pains in the first stage, which occupied upwards of ten hours, were short, harassing, and inefficient; in the second stage they were equally inefficient. The head descended slowly into the pelvis, but did not advance further, and finally, after the lapse of fifteen hours from the

commencement of the labour, I applied the forceps, and delivered her of a living child. Ergot had been previously given, but sickness of the stomach rendered it useless. Immediately after the birth of the child some hemorrhage occurred, and I removed the placenta by pressure. Its expulsion was followed by the most copious and alarming hemorrhage I ever witnessed. The blood gushed out in such a copious stream, that the bed was saturated and the floor deluged with it, in a shorter space of time than it has taken me to pen these lines. My hand all this time was on the uterus. In a few minutes life seemed extinct in this previously debilitated and exhausted woman. As rapidly as I could, and without having recourse to any of "the ordinary means," I injected some ounces of a diluted solution of the perchloride of iron. It acted at once—the hemorrhage was arrested, and my patient slowly revived. On the third day she was wonderfully well, and continued so to improve till the tenth day. Then she had a rigor, the pulse rose rapidly to 160 in the minute, well-marked symptoms of peritonitis subsequently set in, and she died on the fifteenth day after delivery.

Case 4.—Mrs. L., a delicate lady, whose health had been very bad during pregnancy, gave birth to her first child on the 9th Oct., 1871, after a labour of eighteen hours' duration, a living child. The second stage occupied two hours. The child presented with the feet. After the expulsion of the placenta hemorrhage set in, as in Case No. 2. Notwithstanding the use of "the ordinary means," the hemorrhage continued; the uterus would contract, but not firmly, and a constant stream of blood trickled from the vagina. The patient was greatly exhausted, and became very faint. I injected, as in case No. 2, about four ounces of the solution of the perchloride of iron, in the proportion of two parts of water to one of the liquor. The uterus instantly contracted firmly, and no more blood was lost. This patient, considering her constitutional delicacy, made a good recovery, and became pregnant again early in last year. Her labour on this occasion forms—

Case 5.—Labour on this occasion set in soon after midnight on the 27th October, 1873. During the whole of her pregnancy her health had been so bad that I looked forward with much apprehension to her confinement. She suffered, from the very commencement of utero-gestation, from constant sickness of the stomach. This was not mere nausea, with occasional vomiting, but almost everything swallowed was rejected. She had besides the most absolute loathing for food. The tongue was also coated with a thick yellow fur. The bowels were alternately obstinately constipated or violently relaxed. She had, in addition, two severe attacks of illness—

the one of inflammatory sore throat; the other of inflammation of the ear, terminating in abscess. For weeks at a time she was supported by means of enemata of beef-tea and brandy, and more than once during the course of her pregnancy I seriously considered the propriety of inducing premature labour. Labour set in soon after midnight on the 27th October. The first stage was very tedious, and occupied upwards of twenty hours. So utterly inefficient were the pains that I formed the opinion that the uterus opened by a process of mere relaxation, no appreciable pressure being brought to bear on it by the uterine contractions. As soon as the os uteri was fully dilated I ruptured the membranes, and commenced the administration of ergot—not with the view of hastening the labour, but of preventing the occurrence of post-partum hemorrhage. The drug was, however, vomited. The head did not enter the brim; it appeared simply to rest on it—the short, inefficient pains being altogether powerless to cause its advance. I, therefore, applied the long forceps. This was effected with ease. I extracted very slowly; the operation occupied, on this account, nearly forty minutes. The pelvis was roomy and the child small; no difficulty, therefore, was experienced in extracting the child, which, though very feeble, ultimately lived. The uterus, somewhat to my surprise, contracted well. Mindful of the tendency to post-partum hemorrhage exhibited after her former labour, I had, before I applied the forceps, not only administered ergot in combination with strychnia, but had in readiness, in addition to cold water, &c., a vessel containing two ounces of the liquor of the perchloride of iron, diluted with four of water; but my precautions seemed needless. The placenta was in due time detached and expelled, with the aid of gentle pressure applied to the fundus. After its expulsion I still continued for some time to keep up pressure with my left hand on the uterus. As it continued firmly contracted, I then applied the binder, with pads under it, firmly. All seemed so well I thought I might soon with safety leave the patient, when, after the lapse of more than half an hour, she began again to vomit—a copious stream of blood instantly issued from the vagina, and before I could unpin the binder the uterus was so relaxed as to reach above the umbilicus. The pressure I applied seemed only to increase the flow of blood, without exciting any contraction. In five minutes my patient was almost pulseless. This seemed to my mind one of those desperate cases in which I dare not lose time by the employment of what I was satisfied in her case would prove to be inefficient remedies. I therefore at once injected about four ounces of the solution I had fortunately previously prepared. The hemorrhage was at once arrested,

the uterus contracted, but so nearly had life been extinguished, that two hours elapsed before the pulse returned with any degree of firmness to the wrist, or that I dared to leave the bedside of this patient; and so great was the subsequent prostration, that for two days she lay in a state of semi-unconsciousness. She swallowed mechanically beef-tea, brandy, &c., when placed to her lips, but never spoke except when roused. The urine was drawn off with the catheter, the bowels did not move, there was not any attempt at the secretion of milk. Her condition, however, slowly improved, no bad symptoms occurred, and she regained in time her usual health. I understand that she is now (March, 1874) again pregnant.

The foregoing five cases occurred in my private practice, and I had an opportunity of judging not only of the previous state of health, but of tracing the subsequent history of each patient. I desire to comment on some points which appear to me of importance, and as calculated to guide us—first, as to the class of cases in which the injection of a styptic into the uterus for the arrest of post-partum hemorrhage is likely to be most useful; and secondly, as to its subsequent effects on the patient.

1st. It is noteworthy that the only cases which seemed in my practice to demand this treatment were women in a previously bad state of health. Case No. 1 was that of a lady who not only suffered from sickness to an excessive degree during pregnancy, but who for a long time previous to, and of course also during her pregnancy, consumed almost no food, and what she did take was of an improper character. No. 2 was markedly anemic. No. 3 was in such bad health as to cause much alarm to her friends on this account prior to labour. Cases Nos. 4 and 5 were the same patient. She, too, was on both occasions in a very bad state of health—so bad indeed, that the induction of premature labour seemed more than once demanded. In all it may be fairly assumed that the blood was in an abnormal condition, probably destitute of its proper proportion of fibrine. This seems specially likely to have been so in Case 2, in which, though the uterus contracted fairly, the hemorrhage continued.

2nd. As to the results.—In three of the four patients pregnancy subsequently ensued; this fact proves clearly that the injection of the perchloride of iron in no way injured the uterus.

In four of these five cases, notwithstanding previous bad health and the great loss of blood sustained at the time, no unpleasant symptoms of any kind subsequently presented themselves. In one case death ensued. Taking into account her previous ill-health and the acknowledged tendency which

always exists to the occurrence of peritonitis after excessive losses of blood, it hardly seems a reasonable inference that in her case death was due to the effects of the injection of the styptic. The Society have, however, before them all the facts which I am possessed of, for no post-mortem examination was possible. My own opinion is that this patient would probably have died whether the perchloride had been injected or not. Pyemia, phlebitis, and peritonitis have, as is well known, carried off numbers of patients who have suffered from post-partum hemorrhage, long before the injection of a styptic for its arrest was proposed, the debility resulting from the loss favouring the occurrence of these forms of disease; and in the case of the patient under consideration, the state of her health previous to labour aggravated the danger, to which all cases of hemorrhage are liable. But even were it proved that her death was the result of the use of the perchloride, a further question has yet to be decided—namely, this, believing as I did, and still do, that this patient would have died from hemorrhage, and that in a few minutes, was I justified in using an agent which alone, in my opinion, was capable of saving her life? supposing it to be proved that in a certain proportion of cases the use of that remedy would be followed by fatal results.

This question seems to me to be identical with that which is involved in deciding on all capital operations, notoriously in that of ovariectomy, and that it must be decided on the same principles. I shall not, therefore, discuss it further.

For myself I have arrived at the following conclusions:—

1st. That cases of post-partum hemorrhage occur in which the injection of the perchloride of iron, or some similar styptic, is alone capable of arresting the hemorrhage.

2nd. That the injection of such styptic does not necessarily increase the tendency which exists in such cases to the occurrence of pyemia, septicemia, or peritonitis.

3rd. That this treatment is specially applicable to anemic patients.

4th. That while it should never be had recourse to unnecessarily, it should not, on the other hand, be delayed too long.

I may add that in using the solution of the perchloride of iron, I carry out in the main the directions given by Dr. Barnes. I have not, however, in any case injected more than six or eight ounces, sometimes as little as four ounces of the fluid. I also use it somewhat stronger than he does—namely, in the proportion of one part of the strong liquor, B.P., to two of water. The important point in using it is to take care that the end of the tube is passed up to the fundus of the uterus, and that the fluid be injected slowly. I should add that I have

not met with any case in which the uterus did not immediately contract firmly on the perchloride being injected. I am inclined to attribute this to the fact that I had recourse to the remedy before the powers of the patient were so exhausted as to render the uterus incapable of responding to the stimulus.

Before concluding this paper, I desire to say a few words as to what are "the ordinary means" employed for the arrest of post-partum hemorrhage. They are the administration of ergot, pressure and friction on the fundus of the uterus, and the application of cold; these, with the addition of the free exhibition of brandy or other stimulants, may, I think, be considered the means ordinarily had recourse to by practitioners.

As to ergot, it is a most uncertain agent, and while most useful if administered some time before the occurrence of the hemorrhage, is, in my opinion, seldom of much value if given after it has set in. Ergot takes at least twenty minutes to act, and besides is often in these cases vomited. Injected hypodermically it is, I believe, capable of doing much good, but its irritating properties, when thus used, render this method of employing it not altogether unobjectionable. I am at the present time engaged in endeavouring to obtain an efficient and, at the same time, unirritating preparation of this drug for hypodermic injection. As yet I have been unable to obtain any definite results.

The value of pressure on the fundus, if it be judiciously applied, can hardly be over-estimated; but the most carefully applied pressure, or pressure combined with friction, will at times fail to check the flow—in spite of all, the bleeding will go on.

Cold, one of the most potent means at our command for stimulating the uterus to contract, is frequently useless—nay, more, often positively injurious in consequence of its being improperly used. To do good it must cause a shock. It must, therefore, be applied suddenly while the patient is warm. Its application should never be unduly prolonged, for if once the temperature of the body be so reduced that cold, no matter how applied, fail to produce reaction, the uterus will not contract, and the hemorrhage will be in no way checked. Therefore, while I am not prepared to say that cold water should never be poured from a height on the patient, I decidedly object to such a practice, for it necessitates the exposure of a large surface of the body, and the saturating and therefore changing of the bedding. Injecting cold water into the rectum is generally a safe and often an efficient method of employing cold; but, to be of use, it requires to be done before the patient has become exhausted. Injecting water into the

uterus is, I believe, on the contrary, by no means a safe practice. The advocates of this practice tell us that "the injection (of cold water) should be continued till the fluid returns clear." This procedure is, in my opinion, far from being free from danger. I believe it to be quite as likely to be followed by serious consequences as the injection of the solution of the perchloride of iron, while it is far less efficacious. If had recourse to at all it should be done early, and the quantity injected should be but small. Cold water should never be injected into the uterus of a patient exhausted by excessive loss of blood.

Ice introduced into the uterus or rectum will, if the patient be not too much exhausted, cause the uterus to contract. But how seldom is it possible to obtain ice at the moment, and, if attainable at all, much time must generally elapse before it is at hand, and then it is in general too late to be of real good, for, as in the case of the injection of cold water, to be of use it should be used promptly. If it does not succeed at once, its further use can only be productive of mischief. It therefore cannot, for many reasons, be relied on as an efficient agent for the checking of post-partum hemorrhage.

Of the other means advocated for this purpose, and which cannot be classed among the "usual" ones, the introduction of the hand into the uterus is that which is most frequently advocated. There can be no doubt but that in some cases this treatment has proved efficacious. On the other hand, it certainly cannot be relied on. Thus, to quote reliable authority, Drs. Hardy and M'Clintock, give the particulars of a case which proved fatal from the loss of blood, and in which the hand had been introduced into the uterus. The recorded cases in which this plan has been adopted are so few, and the termination of the case, even when the hemorrhage has been checked, so uncertain, that no positive inference as to its value can be drawn. But for myself, I have always looked on the method as one not free from danger. The introduction of the hand into the uterus is far from being a perfectly harmless operation. In this opinion I am borne out by the fact recorded by Drs. Hardy and M'Clintock, that Dr. Charles Johnson, the then Master of the Lying-in Hospital, looked on this practice with great disfavour, and Dr. Johnson's opinion carries great weight, at least with such of us as remember that able practitioner and accurate observer.

My object in making these concluding remarks has simply been to show that the means at our disposal for the arrest of post-partum hemorrhage are far from being reliable, and of what importance it is to add to their number one so powerful as the injection of a styptic solution.—*Obstetrical Journal*, May 1874, p. 107.

98.—ON THE INJECTION OF PERCHLORIDE OF IRON INTO THE UTERUS FOR THE ARREST OF POST-PARTUM HEMORRHAGE.

By Dr. EVERY KENNEDY, President of the Obstetrical Society of Dublin.

[The following remarks of Dr. Kennedy on the new practice of applying perchloride of iron are most important and true.]

Dr. Kennedy wished it to be distinctly understood that, although, as they were aware, he was not averse to the introduction of any improved system of treatment that might be applied to the present or any other branch of obstetrics brought before the Society, yet he must warn them that it was very hard to teach an old dog tricks. It appeared to him that one of the chief advantages of having an experienced man in the chair was, that, in case of novelties being introduced, it was for that man to give his opinion honestly and fairly on the merits of any novel practice that might be brought forward; more especially by contrasting its advantages and disadvantages with those confirmed, in his own mind, by his own experience, as well as by the experience of his predecessors and contemporaries. At the same time it was equally his duty to warn them that, whilst on the one hand they were liable to fall into error by hastily grasping at every novelty that might spring up, on the other hand they should bear in mind that the greatest obstacle to the progress of knowledge was scientific incredulity, and the inveterate conviction, on the part of those claiming to be men of experience, that their knowledge could not be improved upon. With this prelude he asked them to take at its value what he was going to say. He could look back for a long time of hospital and of private practice in the treatment of hemorrhage, and having refreshed his memory by reference to his notes, he could not bring to mind a case of fatal hemorrhage in his private practice but one, and that was in the wife of a medical man. He thought, therefore, the alarm which had been got up on the subject as to its frequency, at least in private practice, was unfounded. The President added that he had little doubt the judicious rules laid down for securing the contraction of the uterus by the late Dr. Joseph Clarke, and insisted upon by Dr. Collins, had much to do with the infrequency of *post-partum* hemorrhage, and if persisted in would lessen these cases. Be that as it may, it came to this, that whatever system was in practice to check hemorrhage in his time, must have been such that the risk was not so great as was now supposed, and that it must have been effectual in preventing hemorrhage. He hailed with pleasure every effort to add to our resources, but he apprehended the result of a rush upon

this plan of treatment—a treatment, in his opinion, not unattended with risk, for if every man who met with a dash of hemorrhage resorted to the use of this styptic, to the neglect of the tried and hitherto successful modes of treatment, the consequences might be most deplorable. It had been stated by Dr. Ringland that out of forty-five cases brought before this Society, treated by perchloride of iron, eleven, or one-fourth, had terminated fatally. He had no hesitation in saying that that was a large proportion of fatalities in hemorrhage. What were the causes of hemorrhage after delivery? Imperfect and irregular contraction of the uterus, and morbid adhesions of the placenta to the uterus. The want of contraction was the main cause. In morbid adhesions there was an altered state of the parts that prevented its efficient contraction, at least where the diseased placental structure had adhered; and in the other case there was imperfect action. The object, then, was to cause the vessels to contract, and generally this was effected by grasping the uterus, following down its contraction by pressure, padding, and bandaging, and by not too rapidly extracting the placenta where this is still retained. In 99 out of 100 cases, this proceeding would be found effectual. But where the circular fibres contracted, and there was a chamber locked up above, how could any styptic applied prevent the hemorrhage. It was possible to account for the efficacy of the styptic where there were altered structures from morbid adhesions, or where there was anæmic inaction. The nature of each case would, perhaps, eventually determine the treatment. A great deal had been said as to the risk consequent on the introduction of the hand. He agreed with Dr. McClintock in thinking the practice not justifiable except the conclusion was arrived at that the hemorrhage could not be otherwise controlled. In such a case it was clearly justifiable, and there was no difficulty about it. He had been obliged in some instances to keep his hand in the uterus for 20 or even 40 minutes, and he believed if he had withdrawn his hand life would have been lost. If they had a case of hour-glass contraction to deal with before or after the expulsion of the placenta, the proper course was to introduce the hand, overcome the spasm of the circular fibres cautiously, thus dilate the contracted portion so as to admit of the contraction of the longitudinal fibres, and remove the clots when the uterus would contract, gradually closing upon the hand. When they came to decide between the use of the hand and an irritating stimulant thrown into the uterus, it should be recollected that the perchloride continued to act as an irritant whilst it remained in the womb. The hand, on the contrary, was withdrawn, and the irritant removed, after effecting the object for which it was introduced;

and why should the hand, when used with caution, cause more injury than the head or breech of a foetus—the natural stimulant inducing uterine contraction. He had not much experience of the perchloride in *post-partum* hemorrhage, but he had been repeatedly obliged to use it in other cases of uterine hemorrhage. He did not look upon it as an innocuous application, and he warned them of this, as he had traced more than two or three deaths as occurring even in minor operations after its use. He stated that distinctly. Taking into consideration the chance of pyæmia, he had no hesitation in saying that his experience went to prove that the use of this application to the interior of the uterus added to the risk in uterine operations, and that the fatality was increased in them by its use. He would not, therefore, have recourse to it unless it was urgently required, believing that other modes of treatment were safer, and, generally speaking, more effectual; but he admitted that cases might occur in which ordinary modes of treatment would prove unavailing, and in such cases he would grasp at the perchloride, and have no hesitation in using it as an additional or *dernier ressort*.—*Dublin Journal of Med. Science*, May 1874, p. 469.

[In all these discussions no reference is made to plugging. How is this?—EDS.]

99.—TREATMENT OF POST PARTUM HEMORRHAGE.

By Dr. BLENNERHASSETT ATTHILL, Waterbeck,
Ecclefechan, N.B.

There exists one plan of treatment, for which I do not claim any novelty, but simply more importance than it at present enjoys. It is a plan for conducting the third part of labour—"all the circumstances which relate to the separation and expulsion of the placenta," according to Dr. Denman. It was my good fortune to become early impressed with this plan of treatment, which I am about to describe; and it is my firm conviction that by its means we can, with an amount of certainty otherwise unattainable, prevent the supervention of that most fearful phenomenon, "flooding." I have never had a case of flooding, although during the management of many varieties of labour, both natural and difficult, and obstetric operations, *post-partum* hemorrhage has threatened; and this I attribute solely to this way of conducting the removal of the secundines. The following is the plan I uniformly adopt. *Immediately* on the expulsion of the child—the binder of course being applied during the commencement of the second stage, and gradually tightened—I pass my hand (the left one), and

grasp the uterus over the abdomen, and make firm pressure downwards in the axis of the pelvis. I then hook in my right index finger on to the edge of the placenta, which is invariably by this time in the vagina; and, with a gentle rotatory movement, extract. If, however, as is very seldom the case, the secundines be not low enough, I follow up the funis, and hook my finger at its insertion, and so effect its removal. I say, immediately the child is born I do this, because, if pressure be not directed at once to the uterus, the womb closes over the after-birth and prevents its speedy expulsion, and a proportionate amount of risk of *post partum* hemorrhage is involved. I do not believe it right to teach that we should wait for a griping pain or two to effect the expulsion of the after-birth. These so-called griping pains should only cause the firmer contraction of the womb *after* the expulsion of the placenta. The advantages of this plan of treatment are the following. 1. We effect the speedy and safe removal of the placenta, and so place the uterus out of the danger of hemorrhage. 2. Should *post partum* hemorrhage set in, we can have the uterus more immediately under our control. 3. It hinders the supervention of an unusual amount of vascular excitement. I submit these imperfect remarks to the notice of the profession, in the hope that they may evoke some larger experience. But I would feel like that deep thinker who, with more meaning than will disclose itself at once, said, "Already my opinion, my conviction, gains infinitely in strength and sureness, the moment a second mind has adopted it." I am sorry that I am unable to state from whose writings I gained this valuable piece of knowledge of the treatment of the third stage of labour and the prevention of flooding; but the subject was treated of in Braithwaite's *Retrospect of Medicine*. Dr. Churchill, in his fifth edition of the *Theory and Practice of Midwifery*, says: "I have adopted this practice for twenty years, and rarely have the placenta detained more than five minutes; nor have I had a case of hemorrhage during that time. If we wait some time before making pressure, the uterus closes over the after-birth, enough to retain, but not enough to expel it."—*Brit. Med. Jour.*, April 18, 1874, p. 507.

100.—ON PUERPERAL PYÆMIA, ETC.

By Dr. J. MATTHEWS DUNCAN, Physician to the Royal Maternity Hospital, and Lecturer on Midwifery in the School of Medicine, Edinburgh.

No theory of this subject can be regarded as final or sure. But the time has come when obstetricians should try to leave off the use of the convenient term puerperal fever, because it embodies error. There is nothing essentially puerperal

known in it; nor is there anything of the nature of a fever, as that term is generally understood. A new name, already widely used, is to be found in the already comparatively old term, pyemia. This new name can be of only temporary utility, but that utility will be very great, and continue till advancing science displaces it by a better, as it should now displace puerperal or childbed fever. It will then have served its time by carrying the ideas of generations of practitioners away from the old, flimsy, and extensively erroneous speculations of the past to the more substantial of this day. It is not to be supposed that pyemia is a term to be analysed into its component parts and held as implying purulent blood. That was once the meaning of pyemia, but it is not so now. The crude pathology of Piorry is already almost forgotten, and his term pyemia is used extensively among the best pathologists as a comprehensive word, indetical with or including the septicemia and ichorrhemia of certain others. It sounds like an adoption of humoralist views, but in it there is as much of solidism as of humoralism, and there is in it vastly more of modern science than in the term puerperal fever.

Pyemia occurs in several forms, which are characterized each by more or less peculiar symptoms, but most distinctly by the pathological appearances discovered post-mortem. There is that most widely known when you have septic embola, and scattered abscesses caused by them, and perhaps otherwise also. There is that where you have inflammation of the peritoneum and other serous cavities, including the synovial and endocardial. There is that where the mucous membranes are chiefly affected—the muco-enteritic. And, lastly, there is that where the only results found after death are—alteration of the blood, enlargement of the spleen, the liver, and degenerations of their most important tissues, with similar degenerations in other organs. It is this last which, often rapidly fatal, was described by Helm, and is now often called acute septicemia. These are the cases which the superficial pathology of our young days described as having no post-mortem appearances at all. An autopsy in those days was made by any practitioner, occupied only a few minutes, and the observations made were of corresponding value. Now an autopsy is a matter understood to demand the labour for a long time, often for hours or even days, of an expert. On these fruitless necropsies, where no appearances were discovered and none supposed to be discoverable, was founded, as you will remember, an argument supposed to be of clenching potency in favour of the essential fever character of the disease. But I confess I have never been able to discover either the logic or the power of the demonstration.

Many researches of different kinds have contributed, and are contributing, to converge scientific light on this greatest of practical, obstetrical subjects; but scarcely one of them can be regarded as being even yet completed, while some are only well begun. In drawing a conclusion, it will be well to take a glance at these various investigations, with which we may class some more general discussions, such as that of Spiegelberg, on this topic.

Van Swieten, Willis, and many old authors on puerperal fever, regarded it as a wound-fever, and Eisenmann's well known work on the subject, published in 1837, is called "Wound-Fever and Childbed-Fever." The wound which these, and even most recent authors, have in view, is that produced by the separation of the placenta; but it is now well known, and has been often found clinically exemplified, that the disease may begin in a contusion or laceration of any part of the genital canal. Many of you have recognised the beginning of a fatal disease in a diphtheritic state of a slight recent perineal laceration, with surrounding redness and boggy swelling. I have already alluded to the recent advances of our knowledge of the anatomy of the placental wound, but already we know where to look for much more light on this subject. The anatomy of the lymphatics, to which recently Recklinghausen, Klein, Thin, and many others have contributed, is not yet completed for the uterus. We all know the fine-looking and distinct, but very unsatisfactory drawings of uterine lymphatics by Moreau; but we do not yet really know their actual anatomy, though very much progress has been made by the anatomical investigations of Lindgren, Fridolin, and especially of Leopold. The completion of this work will be an addition to the theory of puerperal pyemia.

Following the as yet indefinite notions of a wound-fever, came a further true advance from Boyer, Legallois, Cruveilhier, Tonellè, Dugès, and Simpson, who combined to demonstrate the identity of what would now be called the rough morbid anatomy of patients dying after surgical and after obstetrical wounds.

Then came a grand piece of progress, consisting in the discovery and descriptions of phlebitis and lymphangitis, which we owe to Dance, Duplay, Cruveilhier, Robert Lee, Hecker, and Buhl.

We now reach our own times, and have a still grander progress of our knowledge to record, in the discovery of thrombosis and embolism by Virchow, Kirkes, Cohnheim, Buhl, and many others.

After these come researches whose bearings on this subject are certainly very important, but which are, in many respects, as yet immature and incomplete. We allude to the investiga-

tions as to the potency of septic poisons, connected with the names of Davain, Panum, Kehrler, Thin, and many others; the great and actively progressing researches as to the production, diffusion, and influence of bacteria of various kinds, by Lister, Klebs, Waldeyer, Sanderson, Billroth, Winge, Heiberg, Orth, and many others; the new researches of Sanderson on the infective product of all acute suppurative inflammations; researches also into the distinctive characters of the noxious or septic, and the innocuous bacteria.

Besides all these, many valuable results have been elicited from the analysis of experience in hospital and in private practice. In this way the influence of age has been ascertained, and especially of immaturity and of advanced life; so also the baneful influence of primiparity and of excessive childbearing, and of twin-bearing; so also the baneful influence of severe and of complicated labour; so also the bearings of the duration of labour; so also the influence of earliness or lateness of attack after delivery; so also the baneful influence of inclemency of season; so also the dangers attending hospitals and the dangers of communications between the sick and the healthy.

But all of these researches, whether finished or still imperfect, do not, when combined, complete our modern view of this great subject. It is a subject in practical medicine, and we are practitioners. The great object of our work is to prevent or to remove the disease in our patients, and we have to inquire what fruits our knowledge produces for the comfort or healing of the sick.

Many remedies for puerperal pyemia have been proposed, and their successful application loudly proclaimed and widely believed. Doulcet was even rewarded by the French Government for his discovery of the curability of this disease by ipecacuanha. In our own day new cures do not fail to make their appearance, and the advanced knowledge of our times would lead us to expect that they should be more rational, as the phrase is. But who is there of weight in the profession now, who believes in any cure or in any system of specific treatment? All have been found wanting. Yet the wise physician of this formidable disease does not despair of guiding his patient through it, although he well knows its very dangerous character. Experience has shown him the utility of several means for relieving sufferings; and the favourable progress of a case may be encouraged, though not secured, by those invaluable directions which he may give as to diet and stimulants, as well as to more direct medicinal treatment of the genital passage, of the skin, of the bowels, and of the system generally.

But in this disease the physician has long been saying, not that prevention is better than cure, but that prophylaxis or

prevention is to be chiefly looked after, and not cure. Great credit is due to Semmelweiss for the good he has done, especially to hospital patients, by his enlightened zeal in this cause; but the records of hospital practice sufficiently show that much more has yet to be accomplished. Prophylaxis is still farther to be carried out by attention to stop injurious communication between the sick and healthy, by disinfection, and by architectural arrangement, subjects which are all at present receiving much attention from the profession.

In the course of my remarks I have repeatedly referred to scientific researches, as to the poison producing pyemia, and as to the effects of its concentration, and as to the connexion of this poison with the presence and diffusion of bacteria. These researches have been carried on mostly by observations and experiments on the lower animals, and of their very great value there can be but one opinion. But there is a variety of circumstances which seems to indicate that the lower animals are not subject to exactly the same laws in these matters as man is, and certainly there must be great caution exercised in arguing in human pathology from the analogy of the lower animals. The most important of the researches referred to, however—those of Lister and his followers—have been mainly carried out in man, and consist, in a great degree, in the attainment of results in practice equally wonderful and valuable—results that can, so far as we at present know, be attained in no other way. These results go far to justify the belief that pyemia is a septic disease, and that puerperal pyemia may be almost, if not altogether, prevented by the application to delivery of a practice based on antiseptic principles. We know how much has already been, and is, I am happy to say, daily done with success in this direction. But the rules of Semmelweiss, or any other washing of the hands, however carefully conducted, do not constitute treatment according to the manner of Lister. Such imperfect antiseptic precautions, by use of antiseptic gauze and otherwise, I have used with apparent advantage; but we have yet a long way to go, in order to secure complete antiseptic delivery and subsequent treatment. To reach this desirable object, the efforts of several good minds are, I know, directed, both at home and abroad; and some recent unpublished cases of successful antiseptic treatment of wounds of the penis, where periodical discharges of urine have to be permitted, supply a sketch in miniature of plans that might be applied to ordinary confinements. To say more about them I have no right; but I conclude by calling upon you to give your best aid to forward the grand cause of the increased safety of lying-in women.—*Obstetrical Journal*, Sept. 1874, p. 349.

101.—ON PUERPERAL CONVULSIONS.

By Dr. T. MORE MADDEN, Dublin.

There are few obstetric questions, he observed, of greater interest than the etiology, prevention, and treatment of puerperal convulsions. Some advance has been recently made in the prophylaxis and management of this disease, but its causes still remained *sub judice*. Therefore, before relating to the society the history of the cases of puerperal eclampsia which had come under his notice in hospital and private practice, he would first make some general observations on the nature and treatment of this complication of labour, and also briefly refer to the principal theories which had at different periods been applied to the explanation of its causation.

The relative frequency, as well as the danger of convulsions, was shown by a table prepared from the reports of the hospital with which the author had been formerly connected, by which it appeared that out of 50,928 delivered, there were only 138 cases of convulsions. Of these 109 occurred in primipara; 89 of the patients recovered, and 36 died. Of the children 70 were alive and 51 still-born; 57 were delivered by the natural efforts, 39 by the forceps, 20 by craniotomy, 3 by version, and 1 by the vectis.

Dr. More Madden then gave a short summary of the various opinions as to the etiology of the disease, and traced back the theory, formulated by several recent writers, that these convulsions are reflex actions excited by cerebro-spinal or medullary irritation of uterine origin, to Laurence Joubert, Professor of Medicine in the University of Montpellier in the middle of the sixteenth century.

Reflex actions are now generally referred to the medulla oblongata, and modern physiological researches have confirmed the opinions of the older authors in assigning the upper part of the spinal cord, the medulla oblongata, and pons variolii as the probable starting point of the convulsive action in these cases. In proof of the influence of physical impressions on the medulla oblongata in producing convulsive action, Dr. More Madden referred to two cases of acephalous foetuses which came under his observation some years ago in the Lying-in Hospital: one lived for twenty minutes, the other for an hour and a half after birth, and in both it was remarked that the slightest pressure on the bulbous expansion of medulla oblongata, which supplied the place of the brain, produced violent general convulsions.

A considerable number of authorities were quoted to show that the older British obstetricians, with few exceptions, held that puerperal convulsions were occasioned by determination of

blood to the head, and should be treated by blood-letting. It was admitted that pregnancy must be regarded as a predisposing cause of cerebro-spinal congestion, the blood being then increased in quantity as well as containing more fibrine than usual. As gestation progresses the enlargement of the uterus increases the tension of the cerebral vessels, which attains its maximum during the violent efforts of parturition, when puerperal convulsions most frequently commence.

The circumstance of the disease generally beginning at night was also relied on by some as a proof that these convulsions are connected with congestion of the brain. That circumstance, however, did not appear to Dr. More Madden to support that opinion, for, as he had some years previously endeavoured to prove (in a memoir "On Dreaming considered in Relation to the Study of Insanity"), during sleep the brain is in a comparatively anæmic condition, and the blood in the encephalic vessels is not only diminished in quantity, but moves with diminished rapidity.

Convulsions are not confined to plethoric patients, and it was proved that anæmia, whether resulting from the sudden loss of blood by hemorrhage or the gradual deterioration of the vital fluid by disease, is conducive to eclampsia. Nor, continued the author, is pregnancy, even when apparently accompanied by general plethora, actually so in most cases. On the contrary, the blood, though increased in quantity, is then more generally impoverished, containing fewer corpuscles, less albumen, and a larger proportion of water, by which the circulation is more and more embarrassed as the uterus enlarges. This vascular tension occasionally results in serous effusions into the areolar tissue, by which a considerable drain of the nutritive elements of the blood is produced.

The writer then referred to the history of the successive experiments by which it had been shown that the convulsions of pregnancy were frequently associated with dropsy, marked by albuminuria, and attended by the diminished excretion of urea, and the consequent retention of the compounds in the system.

Dr. More Madden, however, dwelt upon the fact that urine is by no means invariably albuminous in these cases. Thus, out of six instances of convulsions during labour, in which he had examined the urine, in only four was albumen discovered, and, on the other hand, he had found albuminuria in pregnant women who had no subsequent attack of eclampsia. In two cases of sthenic puerperal convulsions he had had an opportunity of testing the blood for urea, but in neither was any appreciable amount of this salt detected on a microscopic examination of the evaporated serum treated in the ordinary

manner with nitric acid. Either urea or carbonate of ammonia, resulting from its decomposition, are frequently present, however, to an abnormal extent in such cases; though both these salts may be injected into the blood of a healthy animal without producing convulsions.

From this summary of the most important opinions which had hitherto prevailed as to the etiology of this disease, read by the light of his own clinical observation, Dr. More Madden drew the conclusion that in the causation of puerperal convulsions a variety of circumstances had a share, and must be taken into equal account. In the first place, the disease was obviously connected not only with the state of the uterus itself and with that of the adjoining organs during gestation, but still more so with the remarkable condition of nervous susceptibility peculiar to pregnancy. At this time the pressure of the gravid uterus on the renal emulgent veins must interfere to some extent at least with the functions of the kidneys, as well as act as a cause of cerebro-spinal congestion. And hence in the cases under consideration these cerebro-spinal nervous centres are usually more or less congested, even in those instances in which the patient's general condition is anæmic, and, moreover, are irritated by the circulation of vitiated blood containing some non-eliminated *materies morbi* through the vessels producing a direct toxic effect on the grey excito-motor nerve substance of the brain and medulla oblongata, and stimulating the hyperæsthetic condition just referred to till the latent excitability becomes so intense that it needs only the addition of uterine irritation such as the first pain of labour to cause the pent-up nerve force to burst into uncontrollable action and produce the violent reflex muscular spasms that constitute puerperal convulsions.

It was a remarkable fact that puerperal convulsions generally attack a number of individuals about the same time; thus, of the eight cases which he was about to detail no less than three occurred within a fortnight. It was not improbable, as was long since conjectured by Smellie and by Denman, that the explanation of this circumstance will be found in the occurrence of some peculiar electrical condition of the atmosphere at the time these manifestations of disordered nerve force are most rife.

In almost every instance of this disease that had come under Dr. More Madden's notice the presentation was natural.

Whenever eclampsia occurred towards the end of pregnancy labour was generally produced by the disease. Mental anxiety, especially the combination of shame and despondency peculiar to unmarried women in labour, was clearly the exciting cause of the disease in very many of these cases.

The ordinary classification of puerperal convulsions into

hysterical, epileptiform, and apoplectiform was regarded by Dr. More Madden as misleading and unsatisfactory. The disease under consideration was essentially distinct from either epilepsy or apoplexy, being a convulsive affection *sui generis* peculiar to women who are either pregnant or soon after parturition. The hysterical form of puerperal convulsions is but an attack of hysteria occurring in the early months of gestation, though possibly excited by reflex uterine irritation. The so-called epileptiform and apoplectiform puerperal convulsions are identical in their origin and nature, approaching each other in widely varying degrees in different cases, and influenced in their symptoms by the severity of the attack and the constitutional state of the patient rather than by any essential difference in the nature of the disease. In the majority of instances puerperal convulsions are preceded by œdema of the upper extremities, face and eyelids, pain in the lumbar region, and albuminuria. For some days before the attack the patient generally complains of *malaise*, followed by headache, giddiness, confusion of thought, or peculiar irritability of temper, similar to that which is occasioned by the circulation of lithic acid in the blood and which precedes an attack of gout.

The symptoms of puerperal convulsions in their first stage generally resemble those of an ordinary epileptic fit. Commencing with twitching of the muscles of the face and eyes, the convulsions soon extend to every part of the body, though generally more marked on one side than on the other, recurring at irregular intervals in clonic spasms of increasing violence and duration. In anæmic patients throughout the attack the face may be cool and pale, the eyes glistening, and the pupils contracted. In the majority of cases the patient's state during the commencement of the attack is that of vascular depression rather than of vascular excitement, the extremities being cold, the countenance pallid, and the pulse, though quick, weak and compressible. But generally, as the convulsions recur more frequently, the impeded respiration and consequent non-aeration of the blood induces symptoms of venous congestion. The face becomes dusky and livid, the lips and alæ nasi turgid, the breathing hissing, or stertorous, the pulse full and labouring, and thus the disease passes from the first into the second stage, or from the so-called epileptiform into the so-called apoplectiform convulsions.

In plethoric women the disease generally presents *ab initio* the apoplectiform character, and may commence by a sudden violent convulsion, after which the patient falls into a comatose state, in which she lies, as well described, "like a person dead drunk," the convulsions meanwhile recurring at irregular intervals. Her face is congested, the carotids and temporal

arteries throb visibly, the respiration becomes stertorous, the pulse slow and full, the limbs placid, and no reflex action responds to any external stimulation. After remaining for an uncertain time in this condition—midway between life and death—under favourable circumstances the convulsions may cease, and the patient at last slowly regains consciousness, and awakes once more to renewed vitality, though her mental powers will probably remain clouded for some days. But, on the other hand, the coma may become more profound, the pulse slower and more labouring, the respiration more embarrassed, the face more pallid, the extremities colder, and the skin covered with a clammy moisture, until at length “the last sad scene of all” is closed by a violent and final convulsion.

In all cases prevention is better than cure, and hence the importance of a general recognition of the premonitory symptoms, as by timely prophylactic measures we may sometimes succeed in warding off impending convulsions.

In this prophylactic treatment our objects are—first, to relieve the kidney; secondly, to assist the efforts of Nature to purify the blood after secretions; and, thirdly, to soothe the nervous irritability peculiar to these cases. The first object may be attempted by cupping and fomentations over the loins, the free use of diluents, and the cautious administration of mild diuretics, and especially by colchicum in small and guarded doses; the second intention may be fulfilled by saline aperients, as well as by diaphoretics, if the skin be harsh and dry; and the third by sedatives, especially bromide of potash and belladonna. During the convulsions the ordinary precautions should in the first instance be taken to prevent her injuring herself in any way during the fit.—*Medical Press and Circular*, June 3, 1874, p. 466.

102.—FIBROUS TUMOUR OF THE ANTERIOR LIP OF THE UTERUS—REMOVAL BY THE GALVANIC KNIFE.

Case under the care of Dr. ATTHILL, Adelaide Hospital, Dublin.

A. M., a servant, unmarried, æt. 31, was admitted on the 17th July, 1873. She stated that for some months previous she was conscious of a sense of weight and fulness in the vagina, in all other respects being perfectly well; that more recently, while carrying a heavy weight, she “felt something inside give way,” from which time her discomfort increased, till finally, about six weeks prior to admission, she discovered, after a hard day’s work, a tumour protruding from the vagina. This receded when she lay down, but always reappeared when she stood up or walked about. Menstruation continued to be perfectly normal.

On examination, an ovoid tumour, of the size of a hen's egg, was seen to project from the vagina, its long diameter being parallel with the vulva. In appearance this tumour strongly resembled the unimpregnated uterus, the resemblance being increased by the fact that an orifice, which proved to be the os uteri, could be seen on the upper part of its posterior extremity. Indeed, so great was the resemblance, that at first sight I believed the case to be an example of that very rare affection, complete prolapse of the virgin uterus. The use of the sound, however, proved that idea to be erroneous. It passed upwards, and rather backwards, to the depth of $2\frac{3}{4}$ inches, thus proving that the uterus, though drawn down, was still in the pelvis and but little increased in size. The tumour consisted of the anterior lip of the uterus, which was elongated and thickened; the uterus itself being drawn down by the weight of the tumour till it rested on the perineum, the os uteri being at the vulva. The condition of the parts is correctly represented in the annexed woodcut.

The diagnosis of a fibrous tumour embedded in the anterior lip of the uterus having been made, I determined to amputate the elongated portion of the cervix, electing to do so by means of the galvanic knife, hoping by that method to lessen the risk of hemorrhage, which the thickened and hypertrophied condition of the part led me to think would be likely to occur—an opinion which the event verified. Mr. Perssé White kindly assisted me at the operation. The apparatus employed was Grenet's. The galvanic knife consisted of a loop of platinum wire about half an inch in length, connected by means of the ordinary wire conductors with the battery.

The cervix measured at the point selected for amputation, $3\frac{1}{2}$ inches in circumference. The great thickness of the tissue to be divided, and its extreme denseness, rendered the operation very tedious; it occupied in all thirty-five minutes. The slow progress made in dividing the tissues was also in no small degree due to the vascularity of the part, the flow of blood on each stroke of the knife being so considerable as to cool the platinum wire to such a degree, that ten or fifteen seconds frequently elapsed before the knife was hot enough to be again used. The cauterization was, however, sufficient to prevent any serious hemorrhage occurring; still two arteries had to be ligatured. Excepting what occurred from these two vessels, the cauterization effected by the knife was sufficient to check the hemorrhage, nor was there any subsequent loss of blood. The stump of the cervix cicatrized very slowly, and did not heal perfectly for more than two months. As it healed, the divided surface seemed to be drawn within the cervix, giving me the impression that the tumour had ori-

ginally projected inwards, and that, as it descended, the muscular fibres of the inner half of the cervix uteri had elongated in a greater degree than those of the outer portion.



When discharged from Hospital the uterus seemed to be globular, the cervix being apparently altogether wanting, and this not only as regards the anterior lip, in which the tumour had been seated, but also with respect to the posterior one, which had been in no way implicated. The operation itself was perfectly successful, but the great length of time which it occupied more than counterbalanced the advantage obtained in lessening the risk of hemorrhage. The smell, too, caused by the slow cauterization of the parts, was most disagreeable. As to the amount of pain caused by the galvanic knife as compared with that produced by the *écraseur*, the patient being under the influence of chloroform, no opinion could be formed.

On subsequent examination, the amputated lip was found to contain a perfect fibrous tumour enclosed in its capsule.

Probably in a similar case, a galvanic cautery formed by a band of platinum encircling the part to be removed, and tightened by means of a screw, as practised in similar cases in

America, would prove more satisfactory. — *Irish Hospital Gazette*, Jan. 15, 1874, p. 23.

103.—INTRA-UTERINE STEM PESSARIES.

By Dr. CLEMENT GODSON, Physician to the Samaritan Free Hospital for Women and Children.

It is not my purpose to discuss the merits of intra-uterine stems in gynecological practice, but to describe a new form of stem which I have lately had manufactured for me by Messrs. Arnold and Sons, which I have found very serviceable, and I believe to possess some advantages over other stems in use.

The chief points of recommendation are these: excessive lightness, and a special means of being retained *in situ*, which may or may not be employed.

It is obvious that the less weight there be in the stem, the less likely is it to fall out, or to drag upon the uterus, and therefore the more comfortably may it be worn.

The stems are composed entirely of aluminium, and are so light that if placed on the palm of the hand at some distance from the mouth, they can with very little effort be blown off it.

They are manufactured in five sizes, Nos. 4, 5, 6, 7, and 8; the respective weights of which are 17, 19, 25, 28, and 30 grains. In comparison with the silver stems hitherto employed they are about half the weight. The *largest* aluminium stem, No. 8, is three grains lighter than the *smallest* silver stem in ordinary use, No. 4, which weighs 33 grains, while the largest No. 8 silver weighs 56 grains.

I find, and I attribute it to this property of lightness, that the aluminium stems have as a rule no tendency to get displaced. But in some cases this old trouble arises; to obviate which, I have contrived a spring which can be subsequently inserted; it has simply to be pushed up the canal of the stem, when it springs through the perforation and expands into the cavity of the uterus, thus fixing itself. I advise this spring to be applied only when the simple stem has been tried and failed. There is a certain amount of objection in my mind to anything which projects against the uterine wall. I have been obliged to desist from using the flexible india-rubber stems, because of the hemorrhage occurring and obstinately continuing after their introduction, which has subsided immediately after their removal, and this I believe to be caused by the points, which play against the uterine walls, keeping up incessant irritation. In nearly every case too of ante flexion which I have treated with the flexible stem, I have found, while it has been in place, the uterine body was as plainly to be felt anteriorly as before its introduction. This of course cannot be the

case so long as the aluminium stem remains *in situ*.—*Obstetrical Journal*, Aug. 1874, p. 286.

104.—ON THE DIAGNOSIS OF OVARIAN DROPSY.

[The following is the portion of a review of Mr. Spencer Wells's and Dr. Peaslee's recent editions of their works on ovarian dropsy treating of the diagnosis.]

Mr. Wells describes, with the hand of a master, and with the clearness of one who has himself worked it out, the means of distinguishing between ovarian dropsy and each of the foregoing conditions. He does not hesitate to tell of the cases in which he has failed to make the diagnosis. On the contrary, he records them fully and minutely, and thus lays the foundation for future success. All the senses must be used in obscure cases. Inspection, palpation, percussion, and auscultation, aided by mensuration, must all be used. By the uterine sound much can be learned, as well as by vaginal and rectal digital examination; but, of all these, palpation and percussion afford the most valuable information. By passing the hand over the abdomen, the presence of a tumour is ascertained, also whether it is solid or fluid, or partly the one and partly the other; whether it is movable or immovable, smooth and regular in its outline, or the reverse; but, by percussion, it must mainly be decided whether the swelling is the result of ascites or of ovarian disease, or caused by tympanites, or by a phantom tumour. In ascites, the stomach and intestines, containing air, float on the surface of the fluid, and, therefore, the highest points of the tumour, the patient lying on her back, give out a clear sound on percussion. If, however, the fluid be contained in a cyst, the stomach and intestines are pushed aside, as the tumour rises in the abdomen, and lie in the epigastric and two lumbar regions. Hence, the highest points of an ovarian tumour emit a dull sound when percussed, and the epigastric and lumbar ones give a clear sound. By applying these general rules in any ordinary case, a few seconds will enable a surgeon to clear up all doubt. But Mr. Wells says there are various conditions that may lead to the necessity for further examination. The abdomen may be so much distended by ascites that the intestines cannot float to the surface, the mesentery being too short to allow of their doing so; or the intestines may be tied down by adhesions, the result of peritonitis, probably of a tubercular or, perhaps, of a malignant character, and itself the cause of the ascites. In either case there will be free fluid in the peritoneum, and yet the highest part of the tumour will give out a dull sound on percussion. In another class of cases an ovarian cyst may contain air as the result of a communica-

tion with the intestine, or of decomposition of its contents after tapping, and consequently a clear note will be elicited by percussion. In these cases, Mr. Wells says, physical diagnosis alone will not solve the doubt, and all that can be learned from the history of the case, and general condition of the patient, must be considered in forming a diagnosis. There is another condition that might be classed with those mentioned by Mr. Wells. An over-distended bladder may rise in the abdomen as high as the umbilicus, filled partly with ammoniacal urine and partly with gas, the result of the decomposition of the urine, and present a large fluctuating tumour, dull on percussion in its lower part, and clear at the sides and above, so as to closely resemble an ovarian tumour; but the use of a catheter will at once remove the swelling, and, with it, all doubt as to its nature.

It was for a time hoped that a chemical and microscopical examination of the fluid obtained by tapping might assist the diagnosis. After stating very fully all that may be learned by these methods, Mr. Wells shows that complete reliance cannot be placed in them. Following up the observations of Thudichum on the appearances presented by luteine, the colouring matter of the corpus luteum, Dr. Peaslee hopes for more valuable and reliable information from examinations with the spectroscope, but this part of the subject has not yet been fully explored.

Dr. Peaslee mentions a condition, as occasionally occurring, which we have ourselves so frequently observed that we always look for it as characteristic of the presence of free fluid in the peritoneum, either with or without an ovarian, or other tumour, and we are surprised Mr. Wells does not allude to it—this is a prolapse of the posterior wall of the vagina, from the pressure of the fluid in Douglas' space. It is the upper portion of the posterior wall of the vagina that is prolapsed, the part covered by the recto-vaginal pouch of the peritoneum. It is easily distinguished from the tumour formed by prolapse of the lower part of the vagina known as a vaginal rectocele, by passing the finger into the rectum and thence into the pouch from which the tumour receives its name. We have scarcely ever seen a woman whose abdomen was much distended by ascites in whom this prolapse had not occurred. It is also found where there is an enlarged ovary or other tumour surrounded by ascitic fluid, but never in cases of simple ovarian or other tumour. The umbilicus, too, is often protruded as a hernia by ascitic fluid, either with or without an accompanying ovarian tumour. Mr. Wells states it is often flattened, as in pregnancy, when there is an ovarian tumour, but never prominent unless there be ascitic fluid surrounding the ovarian tumour; but we have ourselves seen at least two cases where the umbilicus was made prominent by the protrusion of a por-

tion of a subjacent tumour. We have also met with a reducible omental hernia at the umbilicus, in connexion with an ovarian tumour, and by this recognised the existence of an adhesion between the great omentum and the tumour, the presence of which was proved during the operation.—*Dublin Journal of Medical Science*, June, 1874, p. 524.

105.—ON CHRONIC OVARITIS.

By LAWSON TAIT, Esq., Birmingham.

Concerning chronic inflammation of the ovary, but little is to be found in the writings of our authorities in gynæcology; and it has only been by the careful grouping of the symptoms of a large number of cases that I have been able to satisfy myself that the condition may be accurately defined and readily diagnosed, and, further, that it may be successfully treated in the majority of cases.

As I have already indicated, chronic ovaritis may be a later stage of moliminal hyperæmia. It may also be the result of acute ovaritis, but the majority of the cases occur from sexual excess and masturbation, or as a sequelæ of exanthemata and rheumatic fever, and probably of syphilis. I have only once had an opportunity of dissecting a case where I had recognised chronic ovaritis in life, and then it certainly was the result of acute rheumatism. It occurred in the case of a girl 17 years old, who had suffered from eight or nine attacks of rheumatic fever. In two of them, she was under my care as a dispensary patient; and, after the recession of the articular affection, an attack of pelvic pain came on, which was increased by pressure, and the attack was accompanied by an irregular menstrual flow. The whole passed off in a few days after the application of a blister, but ever afterwards her menstruation was irregular, profuse, and painful, and she suffered more or less from the symptoms I shall describe immediately. I regarded the attack as one of mild acute or subacute ovaritis, followed by a chronic stage. She died subsequently of embolism of a cerebral artery, and I found her ovaries large, soft, covered with lymph, and dotted with enlarged follicles, and the peritoneum thickened round them. The left ovary was partly adherent to the rectum, and it had nearly the whole of the fimbriæ of the corresponding tube glued on to it.

There is probably a chronic ovaritis of occasional occurrence in chronic phthisis; for, though the rule in that disease is to have ovarian atrophy, evinced first in dysmenorrhœa and finally in amenorrhœa, yet I have seen a few cases where the menstruation was profuse, irregular, and characterised by the other

symptoms of chronic ovaritis. I have seen such conditions temporarily after small-pox, and frequently after scarlet fever in adolescent women. One case I have also satisfied myself of in early acquired syphilis. There is a distinct form of syphilitic metritis, as pointed out long ago by Mr. Langston Parker, and no doubt in these cases the ovaries are involved. Chronic metritis, the result of subinvolution or other uterine accident, and chronic endometritis from catarrh or gonorrhœa, in all probability have some amount of accompanying chronic ovaritis.

Out of eighty-one dissections, Henning found the ovaries diseased in fifty-three cases, and of the latter number six had exudation over them that was, in all probability, inflammatory. Chronic ovaritis, then, is not an uncommon disease. Chronic ovaritis is very often unilateral, and in these cases it probably has had an origin more or less independently of the uterus. Thus, as the result of acute septic ovaritis, it has been, in my experience, invariably unilateral. When it is the result of sexual excess or moliminal hyperæmia, it is generally, though not invariably, symmetrical.

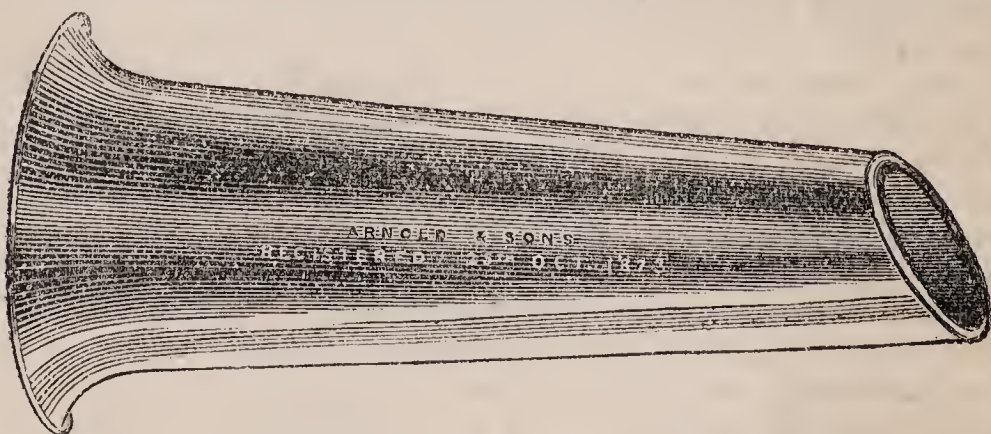
The symptoms which, in my experience, have enabled me to class a number of cases together as chronic ovaritis are, first, in the history of the case, that from the menses the periods have been irregular, generally too frequent, and that they have been too profuse. If the affection have a subsequent origin, then there can be obtained some story of a reason for the disease, either in a gonorrhœal infection or a puerperal accident leading to acute inflammatory attack, or an overindulgence in sexual congress. There is always a sense of weight and fulness in the ilio-hypogastric regions, and there may be positive tympanitic swelling. The discomfort is often so great, that the patient cannot bear her stays tightened or the weight of her dress. Sickness and nausea are frequently present, and almost always for a few days before the menstrual periods. The sufferings from sick headache are often intense. There is always more or less tenderness on pressure over one or both ovaries, and this is invariably increased before, during, and after the catamenia. By tactile examination, this tenderness may easily be demonstrated to be ovarian, and in very many cases the ovaries may be found to be enlarged and tender by the bimanual method of examination. Sometimes examination by the rectum permits a better investigation, especially if the patient be anæsthetised. As a rule, there is not much menstrual pain; for the uterus, sympathising in the disturbance, is often enlarged, and the discharge comes away almost painlessly.

The treatment should consist mainly of organic and systemic rest as perfect as possible during the menstrual periods, and

the administration of ergot. Between times, counterirritation may be used with advantage, and the best form of that I have found to be painting a circumscribed spot of skin in the inguinal region, about two and a half inches in diameter, with linimentum iodi every morning as long as the skin will bear it. When the spot has become too painful to allow a repetition of the painting, the cuticle is allowed to peel off and the skin to become firm, and then the process is repeated as often as may be found necessary. I have had patients going on with this for months, and it nearly always does them good. Further, I give bromide of potassium internally, sometimes combined with ergot. I have also found arsenic and cod-liver oil very useful, and one case yielded to large doses of quinine when everything else had failed.—*British Medical Journal*, June 6, 1874, p. 734.

106.—TAPERING METALLIC TUBULAR SPECULUM.

This speculum has been manufactured by Messrs. Arnold and Sons, 35 and 36, West Smithfield, in accordance with suggestions made by Dr. J. Hall Davis. The advantages which this instrument presents over Ferguson's glass silvered speculum are its non-fragility and its tapering form. It is easier of introduction, and the reflective and illuminating power is greatly increased; the opening being much wider, there is considerably more room for operations. The speculum has a trumpet-shaped external opening, the width of the instrument gradually diminishing from this to its smaller uterine aperture. The tube is suitably bevelled off at its uterine end, and the edges carefully rounded. It is made in three sizes, and produced in two combinations of metals—one electro silver-



plated and gilt at uterine end, and the other nickel silver-plated, which resists the action of acids, and will not tarnish. They are considerably lighter and cheaper than any metal speculum hitherto introduced.—*Medical Times and Gazette*.

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